

THE INTENDED AND UNINTENDED CONSEQUENCES OF PUBLIC POLICY

A Dissertation

by

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Submitted to the Office of Graduate and Professional Studies of  
Texas A&M University  
in partial fulfillment of the requirements for the degree of

DOCTOR OF PHILOSOPHY

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August 2016

Major Subject: Economics

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## ABSTRACT

The government implements various programs and policies with the intention of increasing social welfare. While it is important to evaluate the effects of these initiatives, conducting experiments to determine the implications of public policies is usually expensive and in many cases infeasible. In my dissertation, I employ different causal inference methodologies to identify the causal effects of public policies that affect the society at large.

In the first chapter of my dissertation, I study the effect of the Minimum Legal Drinking Age (MLDA) in altering the drinking behavior of individuals and thereby its effect on sexually transmitted diseases (STDs). Results show no evidence of an increase in STDs at the MLDA in the overall population, in racial subgroups, or in counties with the highest infection rates. The second chapter is an evaluation of the effects of the Section 8 housing voucher program on the criminal outcomes of its recipients. Using a housing voucher lottery, we find that voucher receipt increases violent crime arrests mainly for individuals with a history of arrest and for men. I study a specialized police intervention, called the Lethality Assessment Program, which identifies and empowers the most high-risk victims of domestic violence. I find that this proactive approach of law enforcement towards domestic violence incidents reduced female homicide victimization by men by 35 to 45 percent. Overall, my dissertation sheds light on the effectiveness of public policies and their intended and unintended consequences.

## DEDICATION

To the loving memories of my grandfather, Mr. K. Krishna Rao, and my grandmother,  
Mrs. B. M. Vimala Bai, who always found great joy in my success.

## ACKNOWLEDGEMENTS

First and foremost, I thank my dissertation advisor, Dr. Mark Hoekstra. Mark, thank you for always setting the bar high and telling it like it is. Through the countless hours of meetings and the multiple revisions of my papers and presentations, I have learnt the tricks of the trade from you. I feel prepared for my academic journey ahead thanks to you.

I also thank my committee members, Dr. Jonathan Meer, Dr. Li Gan, and Dr. Lori Taylor for the invaluable research advice and life lessons. I thank Dr. Steven Puller for picking me as his teaching assistant and giving me the opportunity to learn how to teach econometrics. I thank Dr. Jason Lindo for all the job market resources and for giving me a strong recommendation.

I am grateful to the continued support of Mr. Mark Thiele and Mr. Michael Kelsch from the Houston Housing Authority. The second chapter of my dissertation, “Housing Vouchers, Income Shocks, and Crime: Evidence from a Lottery,” was only possible because of the data they made available.

I would like to note that this chapter is co-authored with Jillian Carr. Jill, I have learnt a lot from you, and you are the best co-author one could as for.

Thanks also go to my friends, colleagues, and the staff in the Department of Economics who have been so helpful. Devika and Priyanka, thank you for always being one step ahead of me and helping me navigate the nitty-gritty of the graduate student life. Lynn, thank you for being so easy to talk to. Mary, Brandi, and Teri, thank you for meticulously sending out those recommendation letters.

My parents, Dr. K. Venkatesh and Dr. B. M. Meera, have been instrumental in every step that has led me to this point. Amma and Appa, thank you for your love, guidance, and faith in my abilities. I also thank my sister, Supriya, for looking up to me and motivating me to always do my absolute best and set a good example. My parents-in-law, Dr. B. S. Srikanta and Mrs. Shashikala, have been very supportive and encouraging throughout my graduate studies. Atte and Maava, thank you.

Finally, I give my heartiest thanks to my best friend and life partner, my husband. Karthik, you have always believed in me more than I ever believed in myself. Thank you for being my rock and putting up with me through the research successes and failures I have had over the years.

Last but not the least, I am grateful for Abhyuday, my dear son and the greatest blessing in my life.

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## 1. INTRODUCTION

Public policies and programs affect various aspects of life for large populations. These policies and programs are implemented with an aim to achieve certain objectives, but oftentimes they might not work as intended or might also have unintended consequences that could be positive or negative. Therefore, it is important for policy makers to evaluate whether the benefit of a policy exceeds the cost of implementing it. While it is usually straightforward to calculate the cost of implementation of a public policy or social welfare program, analyzing its effects on important outcomes is difficult due to the lack of an experimental approach in their implementation. Specifically, in the absence of randomly chosen control and treatment groups, it is difficult to disentangle the causal effects of a program from the effects of other confounding factors that might influence the outcomes of the control and treated groups differently. As a solution, a variety of causal inference techniques have been developed in the field of Applied Microeconomics. In this dissertation, I analyze the causal effects of three public policies/programs using different experimental and quasi-experimental methodologies.

The first chapter of my dissertation studies the effect of the Minimum Legal Drinking Age (MLDA) in altering the drinking behavior of individuals and thereby its effect on sexually transmitted diseases (STDs). The MLDA provides legal access to alcohol to individuals on their 21<sup>st</sup> birthday and has been shown to cause large increases in drinking (Carpenter and Dobkin 2015). However, we cannot compare the STD rates in populations below and above 21 years of age because these groups may also differ in other factors,

such as sexual health awareness and access to condoms, which affect STD rates. To overcome this issue, using confidential administrative data from California, I implement a regression discontinuity model which compares the number of gonorrhea cases in men who differ in age by less than a month, but are diagnosed just before and after their 21<sup>st</sup> birthday. Results show no evidence of an increase in STDs in the overall population, or in racial subgroups and counties with the highest infection rates. This suggests that while the increase in drinking at the MLDA has negative consequences such as higher mortality and crime (Carpenter and Dobkin 2009; 2015), the same is not true for STDs.

The second chapter looks at the effect of the largest federal housing assistance program in the United States, “Housing Choice Voucher Program,” on crime. This program, which is more popularly known as “Section 8,” is designed to provide in-kind transfers to low income families in the form of vouchers so they can get access to privately owned housing which would otherwise be unaffordable. Identifying the causal effect of the vouchers is difficult as families choose to participate in the program and a comparable control group is not readily available. Even among low-income families, those that choose to participate in the program might be different from those that do not in unobservable ways. Taking advantage of a housing lottery that randomized the timing at which families were enrolled into the program, we estimate the causal effects of the vouchers through an experimental approach. The data include information on voucher recipients from the Houston Housing Authority and arrest records from the Houston Police Department. Results show that this form of assistance has unintended consequences. Voucher recipients were more likely to

get arrested for violent crime after receiving the vouchers. Further analysis reveals that these increases in crime are driven by recipients with a history of crime and by men.

In the third chapter, I evaluate a police intervention called the Lethality Assessment Program, which aims to reduce intimate partner homicide. Under this program, the police officers take a more proactive approach at the scene of a domestic violence incident. They use a short, simple questionnaire to screen the victims and identify those most at risk of being seriously injured or killed by their intimate partners. The high-risk victims are then connected to a domestic violence counselor, given a safety plan, and actively encouraged to seek help. I study the effectiveness of this program by exploiting the variation in the timing of implementation of LAP across law enforcement agencies in Maryland, where the program was first developed. Results indicate that the program reduced female homicide victimization by males by 35-45 percent. This translates to a reduction of 2-3 female fatalities annually for a population of 1 million.

## 2. THE EFFECT OF THE MINIMUM LEGAL DRINKING AGE ON SEXUALLY TRANSMITTED DISEASES: REGRESSION DISCONTINUITY EVIDENCE

### 2.1 Introduction

The Centers for Disease Control and Prevention (CDC) estimates that around 20 million new sexually transmitted infections occur each year in the United States, with almost half of them infecting adolescents and young adults between the ages of 15 and 24. The total number of prevalent infections is estimated to be around 110 million, which result in annual expenditures of \$16 billion in direct medical costs (CDC 2013 STD Factsheet). Studies showing positive links between drinking and risky sexual behaviors, such as having multiple partners (e.g., Welsh, Grello, and Harper 2006) and engaging in unprotected sex (Grossman and Markowitz 2005), make it critical to understand the extent to which alcohol use affects the spread of sexually transmitted diseases (STDs).

Researchers in various fields have studied the influence of alcohol on risky sexual behavior. On the one hand, physiological effects of alcohol include decreased testosterone levels which can reduce libido and sexual performance (e.g. Mendelson, Mello, and Ellingboe 1977). On the other hand, psychological effects such as cognitive impairment (Hull and Bond 1986) and relief of social anxiety (Steele and Josephs 1990) might lead to decreased rational judgment in the choice of sexual partners and in the decision to use protection. Increased aggression and sex drive under the influence of alcohol have also been documented (e.g. Hull and Bond 1986). Therefore, estimating the impact of drinking on risky sexual behavior remains an empirical question.

However, establishing a causal link between alcohol consumption and risky sexual behavior is difficult due to selection into drinking. Specifically, the worry is that an unobserved confounding variable, such as a preference for risky behavior, may cause an individual to engage in both drinking and unsafe sex. To overcome this problem, researchers have often used variation in alcohol prices (and taxes) and other alcohol related policies as sources of variation in alcohol consumption.<sup>1</sup>

Whereas, I address this question in this paper by exploiting the sudden change in legal access to alcohol at exactly age 21, in a regression discontinuity (RD) design, to study the corresponding change in the likelihood of contracting an STD.<sup>2</sup> The Minimum Legal Drinking Age (MLDA) grants individuals legal access to alcohol on their 21<sup>st</sup> birthday and has been shown to cause a significant increase in both drinking participation and frequency (Carpenter and Dobkin 2009; Carpenter and Dobkin 2011; Yörük and Yörük 2011; Carpenter and Dobkin 2015). Given these first stage effects, I use administrative count data from California to test for a discrete change in gonorrhea rates in men at age 21. The data cover all cases of gonorrhea in the state of California that are diagnosed in

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<sup>1</sup> Alcohol prices and beer taxes have been used as instruments for consumption to study the impacts of drinking on condom use (Grossman and Markowitz 2005; Markowitz, Kaestner, and Grossman 2005), teenage abortions (Sen 2003), and state level STD rates (Chesson, Harrison, and Kassler 2000; Markowitz, Kaestner, and Grossman 2005). State-level variation in legal drinking ages has been used in a difference-in-differences framework to study the effects of drinking on STD rates (Chesson, Harrison, and Kassler 2000), teen child-bearing (Dee 2001), and infant health outcomes (Fertig and Watson 2009). The effect of the "Zero Tolerance" drunk driving laws on STD rates among the underage population has also been studied in a similar framework using state-level variation in these laws (Carpenter 2005). But, some concerns have been raised about alcohol prices (and taxes) being weak instruments for consumption (Chaloupka and Wechsler 1996; Rashad and Kaestner 2004) and about the timing of the changes to the legal drinking age across states being endogenous (Miron and Tetelbaum 2009).

<sup>2</sup> This identification strategy was first used by Carpenter and Dobkin (2009) to study the effect of drinking on mortality.

men in the age group of 17 to 25 during the time period from 2000 to 2012. Importantly, I observe the exact age (in days) at the time of diagnosis, and gonorrhea in men is typically diagnosed within two weeks from the time of exposure. Since only the time of diagnosis is observed and not the time of contraction, this short time span between infection and diagnosis is critical for this research design.

In exploiting the discontinuity in drinking at age 21, this paper is close to a study by Ertan Yörük and Yörük (2015) (henceforth E&Y) who use the same approach and confidential data from the National Longitudinal Survey of Youth to study the effect of increased drinking at the MLDA on risky sexual behavior. While they find a discontinuous increase in the likelihood of an individual engaging in sexual activity at age 21, they do not find a statistically significant change in the frequency of sexual activity or in the likelihood of using a condom (or any other form of birth control) in the most recent sexual intercourse. This is an important contribution since most of the existing literature has documented a positive relationship between drinking and risky sexual behavior.

This paper differs from and complements E&Y (2015) in several ways. First, the outcomes of interest in E&Y (2015) are self-reported measures of risky sexual behavior from the NLSY, whereas I focus instead on clinically diagnosed STDs. I argue that while it is important to know the behavioral response to increased drinking, the impact on STDs is likely of more direct interest to policy-makers and public health professionals. In addition, an important advantage of this study is that it relies on administrative data, which cover the universe of gonorrhea infections in California diagnosed in males over a 13 year



period, rather than survey responses, and this helps avoid common worries about using self-reported data on a topic like risky sexual behavior.<sup>3</sup>

The advantage of the regression discontinuity research design is that it offers a clean and intuitive way of identifying the effect of the MLDA and the associated increase in drinking on STDs. The identifying assumption is that all determinants of STDs, other than legal access to alcohol, trend smoothly across the age 21 cutoff. This assumption is likely to hold because there is little to no incentive for individuals to manipulate their date of birth at the time of STD diagnosis, and all confounding factors other than legal access to alcohol are expected to be no different, on average, in the populations just below and above the age 21 cutoff. Therefore, any discontinuous increase in gonorrhea rates at this cutoff can be interpreted as the causal effect of the MLDA.

Results show no evidence of an increase in STD rates at the MLDA. All point estimates are negative, and none are statistically different from zero. In addition, I find no evidence of an increase in STDs even within racial subgroups and counties with the highest infection rates. Importantly, the estimates are sufficiently precise to rule out a 1.6 percent increase in the likelihood of contracting gonorrhea due to increased access to alcohol. In short, even though drinking increases significantly at age 21 among California residents, by around 31 percent (Carpenter and Dobkin 2015), the results indicate that this

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<sup>3</sup> For example, one might worry that respondents may not truthfully report risky sexual behavior that arises due to drinking, or that they may not be able to recall the details of risky sexual activity that occurred under the influence. In addition, the survey question in the NLSY only asks about condom use in the most recent sexual activity in the last four weeks, which may be a noisy measure of overall condom use. The NLSY also does not ask about the number of sexual partners. As a result, even if condom use does not change, individuals over age 21 who have risky sex may be doing so with more number of partners and thus be at a greater risk of contracting an STD.

additional drinking does not result in an increase in gonorrhea infection rates. The absence of an increase in STD rates at the MLDA of 21 years casts a doubt on the hypothesis that alcohol consumption, rather than a confounding factor, causes the spread of sexually transmitted infections.

## 2.2 Background and Data

One of the challenges in implementing an RD model to estimate the impact of the MLDA on outcomes is that researchers must observe the age of the individual at the time of occurrence of the outcomes. For example, in studying the effect of alcohol on mortality, Carpenter and Dobkin (2009) are able to calculate the exact age in days at the time of death with date of death and date of birth available to them. However, doing this in the case of STDs is difficult since the data typically only contain the date of diagnosis and not the date of contraction of (or exposure to) the infection.<sup>4</sup> Since STDs may have long and unpredictable incubation periods, which also vary by gender, estimating the exact age at which the individual engaged in unsafe sex from the age at diagnosis is not straightforward. Given these difficulties, I focus on gonorrhea in men as it is diagnosed, in most cases, within a short time span of two weeks after exposure to the infection.

Gonorrhea is a sexually transmitted bacterial infection. It is the third most common STD affecting teenagers and young adults in the age group of 15 to 24.<sup>5</sup> According to CDC estimates, about 820,000 people are infected with gonorrhea annually in the US, of

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<sup>4</sup> Even if such a date is available in the data, it is likely to be self-reported and may not be reliable.

<sup>5</sup> The first two most common sexually transmitted infections in this age group are HPV and Chlamydia. Since these diseases are not usually diagnosed within a short period of time after exposure, they cannot be studied in an RD setting.

whom 70% (570,000) are between the ages of 15 and 24. The symptoms of gonorrhea in men include white, yellow, or green discharge from the penis, burning sensation while urinating, and painful or swollen testicles (Gonorrhea CDC Factsheet).<sup>6</sup> These symptoms are difficult to ignore, and most or all of them appear within one to fourteen days after the exposure to the infection.<sup>7</sup> Given the severity of these symptoms, we can expect men both below and above age 21 to be equally likely to seek medical help when they appear. Hence, there would be no reason to believe that a change in the probability of testing could explain the observed changes in gonorrhea rates.<sup>8</sup> Gonorrhea is detected through a urine test and is treated with prescription antibiotics.<sup>9</sup> Men who are infected with gonorrhea are, therefore, very likely to be diagnosed within two weeks of contracting it. This short time window between exposure and diagnosis allows me to estimate the age of the individual at the time of contracting the infection from his age at the time of diagnosis. Specifically, the age at contraction is estimated as follows:

$$\text{Age (in days) at Contraction} = \text{Age (in days) at Diagnosis} - 7 \quad (1.1)$$

The ages at the time of contraction are likely to be estimated with a small margin of

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<sup>6</sup> The same is not true for women. Gonorrhea usually does not present any symptoms in women. See CDC Fact Sheet on Gonorrhea: <http://www.cdc.gov/std/gonorrhea/stdfact-gonorrhea.htm>

<sup>7</sup> Seven days is thought to be the typical incubation period for gonorrhea (Crawford et al. 1977). In a preventive drug trial (King et al. 1979), the mean incubation period was found to be between 3.4 and 5.6 days, and among the men who were screened for asymptomatic gonorrhea, only 25 percent of them did not have any symptoms until the fourteenth day.

<sup>8</sup> Some of the men who are infected with gonorrhea may be asymptomatic. An increase in testing probability might cause an increase in the number of such asymptomatic gonorrhea cases detected. This would mean that my estimates indicate the upper bound of the increase in STDs at the MLDA. On the contrary, it is difficult to believe that legal access to alcohol and an increase in risky sexual behavior would cause those who do not show symptoms to be *less* likely to get tested.

<sup>9</sup> These antibiotics are not available over the counter. Since the individual would need a prescription, all diagnosed cases get reported to the CDPH.

error (of  $\pm 7$  days).<sup>10</sup> Also, in some cases, the infected male might be asymptomatic and hence not be diagnosed within fourteen days of exposure.<sup>11</sup> However, there are three important things to note. First, neither the proportion of asymptomatic gonorrhea cases nor this measurement error in the estimated age at contraction are likely to change discontinuously at the age 21 cutoff.<sup>12</sup> As a result, the regression discontinuity estimates remain unbiased. Second, as a robustness check shown later, I estimate all the results with and without the cases diagnosed within the two weeks immediately after the 21<sup>st</sup> birthday. The results are robust to these exclusions. Third, all the estimates are measures of percentage changes in the number of sexually transmitted infections at the MLDA. Though the observed number of cases might be lower than the actual number of infections at a given age due to the cases of asymptomatic gonorrhea, the relative change is unaffected.

The data are from the STD Control Branch of the California Department of Public Health (CDPH). They include information from all cases of males in the age group of 17 to 25 diagnosed with gonorrhea between 2000 and 2012 in the state of California. The total sample size is 67,443. CDPH does not share individual level data due to privacy

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<sup>10</sup> For example, if a male gets diagnosed 3 days after exposure, the error in his estimated age at contraction is -4. For a male who is diagnosed 8 days after exposure, this error is +1. The error will thus vary between  $\pm 7$ . Importantly, this error is likely to be the same on average on both sides of the age 21 cutoff.

<sup>11</sup> In the absence of symptoms, men usually get diagnosed with gonorrhea during routine STD screenings or at times through contact-tracing if they are named as sexual contacts of women diagnosed with gonorrhea. In a study that closely replicated the screening process by conducting routine checkups of individuals who had previously visited an STD clinic, 39.1 percent of the men diagnosed with gonorrhea were asymptomatic (Peterman 2006). In cases diagnosed through contact-tracing, the proportion of infected men who are asymptomatic ranges from 44 to 57 percent (Crawford et al. 1977).

<sup>12</sup> Though it is reasonable to think that the proportion of asymptomatic men diagnosed increases with age, it is not expected to jump discontinuously at exactly age 21.

concerns. As a result, I observe counts of gonorrhea cases aggregated by the exact age in days at the time of diagnosis. The data also include count breakdowns by race and by county groups based on infection rates.<sup>13</sup>

Descriptive statistics are shown in Table A.1. On average, about 23 cases are diagnosed at every age measured in days between 17 and 25 years. Blacks have the largest number of occurrences with an average of about 7.4 cases diagnosed at every age in days. Close to 80 percent of the incidences of gonorrhea in this sample are in the 10 counties with the highest infection rates.

The details of gonorrhea infection rates in California are presented in Table A.2. Panel A shows infection rates among males in 2012 broken down by race and age group. The infection rate for black males is close to five times that of Hispanic and white males. While the reported rates are higher for males than females, this is likely driven by differences in symptoms and probability of diagnosis between the genders. The infection rate for males is the highest in the age group of 20 to 24, with about 1 in every 290 men being diagnosed with gonorrhea in 2012. For black males in this age group, 1 individual out of every 78 was found to be infected. Given such high infection rates, any change in the number of cases diagnosed at age 21 should be discernible. Panel B shows the infection rates across different counties from 2008 to 2012. The top 10 counties in the decreasing order of infection rate are listed. San Francisco had the highest infection rate, while Los Angeles had the highest number of infections.

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<sup>13</sup> Based on the gonorrhea infection rates from 2008 to 2012, counties were divided into 3 groups: Top 3, Top 4 to 10, and the rest. Information at individual county level was unavailable due to privacy concerns.

### 2.3 Empirical Approach

Using the variation in alcohol access and consumption induced by the MLDA, I implement a regression discontinuity design to estimate their effect on the likelihood of contracting a sexually transmitted infection. Formally, I estimate the following equation:

$$STD_{age} = \beta_0 + \beta_1 Above21_{age} + f_1(age) + f_2(age * Above21_{age}) + \varepsilon_{age} \quad (1.2)$$

In this equation,  $age$  is the age (in days) minus 21 years.  $age$  is both the unit of observation and the running variable.  $STD_{age}$  is the log of the number of cases of gonorrhea where the individual was  $age$  days away from his 21<sup>st</sup> birthday at contraction.  $Above21_{age}$  is a dummy variable indicating that  $age$  is equivalent to being 21 years or older.  $\beta_1$ , the main coefficient of interest, captures the percentage change in the number of gonorrhea cases at the MLDA. I also estimate the above equation using a negative binomial regression in which  $STD_{age}$  is the count of gonorrhea cases where the individual was  $age$  days away from his 21<sup>st</sup> birthday at contraction. The interpretation of  $\beta_1$  is the same in both models.

This empirical approach has been used in the past to study the effect of drinking on many outcomes such as mortality (Carpenter and Dobkin 2009), crime (Carpenter and Dobkin 2015), college performance (Carrell, Hoekstra, and West 2011), marijuana use (Yörük and Yörük 2011; Crost and Guerrero 2012; Crost and Rees 2013; Yörük and Yörük 2013), psychological wellbeing (Ertan Yörük and Yörük 2012), and risky sexual behavior (Ertan Yörük and Yörük 2015). The underlying identification assumption of this research design is that all of the determinants of the likelihood of contracting sexually transmitted infections, except for legal access to alcohol, trend smoothly at the age 21 cutoff. This is

a reasonable assumption because, other than legal alcohol access, there is no reason to expect any other determinant of STDs to change discontinuously on exactly the 21<sup>st</sup> birthday of an individual. Also, individuals do not have an incentive to misreport their date of birth when they are diagnosed with an STD. That is, manipulation around the age 21 cutoff in the STD data should not be an issue.<sup>14</sup>

This identification strategy has two main caveats. First, this approach identifies effects for men around the age of 21 years. This group is of special interest given the high rate of STDs among the age group of 20 to 24, but it is unclear whether findings of this study can be extrapolated to men of other ages. Second, although I expect the first-order effect of legal access to be through the increase in drinking, it is possible that changes in social environment (e.g., increased time spent at bars) at the MLDA have their own effect on the likelihood of contracting an STD. In essence, this approach identifies the reduced-form impact of the MLDA on STDs.

## 2.4 Effect of the MLDA on Drinking

With the data from the CDPH, I am unable to directly test for the effect of the MLDA on drinking. However, researchers have consistently shown that alcohol consumption increases on both the intensive and extensive margins at age 21. Carpenter and Dobkin (2009) use data from the National Health Interview Survey from 1997-2005 and estimate a 11 percent increase in drinking participation (the probability of having had a drink in the

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<sup>14</sup> The threat to identification comes from the possibility that certain types of individuals would misreport their age when they are diagnosed with gonorrhea, but this is very unlikely. It is possible that individuals misreport their age to gain access to alcohol before turning 21, but this simply implies a first stage that is smaller than 100 percent.

past year) at age 21. They also find a 21 percent increase in drinking frequency measured by the proportion of drinking days. Yörük and Yörük (2011) use data on individuals aged 19 to 22 from the confidential versions of the NLSY1997 and estimate smaller increases in drinking at the MLDA. The probability of drinking in the past month jumped 6 percent, and respondents who were over 21 reported to drink on 1.5 more days in the previous month than those who were not. Carpenter and Dobkin (2015) use confidential versions of the 2001, 2003, and 2005 waves of the California Health Interview Survey (CHIS) and find a 31 percent increase in the probability of drinking in the month prior to the survey.<sup>15</sup> Drinking frequency is also found to increase; respondents just over 21 reportedly drank on 57 percent more days in the prior month than those just under 21. However, the increase in the likelihood of binge drinking, defined as the consumption of five or more drinks in one sitting, at age 21 is not statistically significant. The estimates of the first stage from the CHIS data are particularly applicable to this paper as the alcohol consumption data are from California and the timing of the surveys falls within the period of this study.

A potential concern with these findings is that survey respondents under age 21 under-report drinking because underage drinking is illegal. However, in the CHIS data, Carpenter and Dobkin (2015) do not find the responses of underage individuals to alcohol related questions to be systematically missing. Additionally, they use data from arrest records to show that there are large and significant increases in many alcohol related

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<sup>15</sup> The first stage results are presented in the appendix of Carpenter and Dobkin (2015). These percent changes have been calculated using the reported point estimates and the levels of the variable just before age 21.



crimes such as driving under the influence and drunkenness at age 21, which are not subject to such desirability bias.

## 2.5 Results

### 2.5.1 *Effect of the MLDA on STDs*

First, I examine the change in the incidence of gonorrhea at the MLDA graphically. Figure A.1 presents the plot of local averages of the number of men diagnosed with gonorrhea in 30-day blocks of age. The fitted lines are quadratic fits of the o of gonorrhea cases by exact age in days on either side of age 21. The count of STD cases trends smoothly across the age 21 cutoff with no visible discontinuity.

I formally estimate the discontinuity using equation 1.2 and do not find any discrete increase in the count of STDs at the MLDA. I test for the robustness of the results to excluding individuals who are diagnosed within the two weeks after their 21<sup>st</sup> birthday. The results are presented in Table A.3. Panel A shows estimates from log-linear regressions where the dependent variable is the log of the count of STDs by age in days. Panel B shows estimates from the corresponding negative binomial regressions. Estimates are presented in the decreasing order of bandwidth (lengths of age on either side of 21 years included in the regression), ranging from 48 to 6 months. Different age polynomials are included in regressions with different bandwidths allowing for the most suitable fits. I control for cubic functions of age on both sides of the cutoff for the 48 and 36 month bandwidths, quadratic for the 24 month, and linear for the 12 and 6 month bandwidths. Results in columns 2, 4, and 6 have the same specifications as those in columns 1, 3, and 5, but exclude the cases diagnosed within zero to fourteen days after the 21<sup>st</sup> birthday of

the individual. This is to ensure that the measurement error in the estimated age at contraction is not driving the results in the full sample.

The results are consistent across all specifications and log-linear and negative binomial models. None of them provide evidence of an increase in STDs at the MLDA. All of the point estimates of discontinuity are negative and are statistically indistinguishable from zero even at the 10% level. Importantly, estimates are sufficiently precise to rule out meaningful increases in gonorrhea. For instance, in my preferred specification of the negative binomial model with the 48-month bandwidth and controls for cubic function of age, the upper bound of the 95% confidence interval (given a two-sided test) of the effect of legal access on gonorrhea rates in men is 1.6 percent.

### *2.5.2 Effect of the MLDA on STDs in Racial and Geographic Subgroups*

While the results above suggest that the MLDA and the associated increase in drinking do not result in an increase in STDs on the whole, there may be differential effects on more narrowly defined sexual networks. Factors such as race (due to assortative mating) and geographic area (by the proximity of potential mates) are important determinants of an individual's sexual network. Firstly, research suggests that asymptomatic gonorrhea may be more common in white men than black men (Crawford et al. 1977).<sup>16</sup> Since the research design hinges on the infection being diagnosed within a short period after exposure, it is important to study the effects within individual racial groups. Secondly, infection rates are markedly different across different counties in California. In counties

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<sup>16</sup> The strain of the bacteria causing asymptomatic gonorrhea was found to be more common among white men and less common among black men.

with very low infection rates, the probability of having a sexual encounter with an infected person might be so low that despite a change in risky sexual behavior, no discernible change might occur in STD rates. Given these differences across subgroups, I estimate the discontinuity in gonorrhea rates at age 21 within racial and county subgroups allowing for possible heterogeneous effects. The data include a breakdown by race (whites, blacks, and Hispanics) and county groups based on gonorrhea infection rates (3 groups: top 3, top 4 to 10, and the rest of the counties).

Results indicate that there is no effect of increased drinking at the MLDA on gonorrhea rates for men in any of these subgroups. Figure A.2 presents the graphical evidence where the counts of gonorrhea by exact age in days are plotted separately for whites, blacks, and Hispanics. Similarly, Figure A.3 presents the plot of gonorrhea counts by age in groups of counties with different infection rates. The counts are found to trend smoothly across the age 21 cutoff, and the visual evidence indicates a zero-effect of the MLDA on STDs within each of the racial and geographic subgroups.

I estimate the discontinuity within each group using equation 1.2. Results for whites, blacks, and Hispanics are shown in Panels A, B, and C of Table A.4. Results for the top 3 and the top 4 to 10 counties by infection rate, and for the rest of the counties are in Panels A, B, and C of Table A.5. In both tables, estimates are from negative binomial regressions, and they are presented in the decreasing order of bandwidth from 48 to 6 months with

controls for different polynomials of age.<sup>17</sup> As in the case of the entire population, there is no evidence of an increase in the incidence of STDs at the MLDA in any of the subgroups. In both Tables A.4 and A.5, all the estimates of the discontinuity across different subgroups and bandwidth specifications are statistically indistinguishable from zero even at the 10% level.

In summary, I find little evidence of an increase in STDs across any of the different sexual networks defined by either race or geography. Importantly, I find no effects for black men for whom the infection is more likely to be symptomatic and infection rates are as high as 12.8 per thousand. Also, I find no effects even within the three counties with the highest infection rates (ranging from 3 to 8 cases per thousand). These results suggest that the lack of an effect of the MLDA on STDs in the overall population is not a consequence of averaging across groups for whom there are opposing effects, but rather that the MLDA and the associated increase in drinking do not cause increases in STD rates in any population.

## 2.6 Conclusion

This paper evaluates the effect of the minimum legal drinking age and the corresponding increase in drinking on sexually transmitted diseases. Effects are identified by exploiting the sudden change in legal access to alcohol at age 21 in a regression discontinuity model. Specifically, with administrative data from the California

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<sup>17</sup> When the data are broken down by race and county groups, the count of STDs is zero for some of the ages (in days). Hence, I prefer the negative binomial model. I also estimated the log-linear models using the same workaround as Carpenter and Dobkin (2009), where the dependent variable was calculated as  $\log(\text{STD count for the particular race} + 0.5)$ . The results from both models are qualitatively similar.

Department of Public Health, I compare counts of gonorrhea cases in men (aggregated by the exact age in days) diagnosed just before age 21 to that just after. Since gonorrhea in men is mostly diagnosed within fourteen days after exposure, any changes in the gonorrhea infection rates around the age 21 cutoff should result in changes in gonorrhea diagnosis rates within two weeks of that cutoff.

Although the increase in drinking at the MLDA, along with increases in arrests for driving under the influence, drunkenness, and nuisance crimes, has been previously documented (Carpenter and Dobkin 2009; Carpenter and Dobkin 2011; Carpenter and Dobkin 2015; Yörük and Yörük 2011), the results of this paper indicate that the MLDA and the associated increase in drinking do not result in an increase in sexually transmitted diseases as measured by gonorrhea infections in men. This is true not only in the overall population but also in racial and geographic subgroups expected to be most at risk of contracting gonorrhea.

The results of this paper differ from those in earlier work that study the relationship between drinking and STD rates. For instance, Carpenter (2005) studies the effects of “Zero Tolerance” drunk driving laws and finds that the adoption of such a law by a state significantly reduced gonorrhea rates among 15–19-year-old white males. Many factors could explain these differences. To the extent that 21 year-olds might have greater sexual awareness and greater access to birth control than teenagers, 21 year-olds might be less likely to engage in unprotected sex overall. In addition, the nature of the reduction in drinking that occurs as a result of Zero Tolerance laws might be different from the nature of the increase in drinking that results from gaining legal access to alcohol at the MLDA.

These results are important to public policy in two ways. Firstly, sexually transmitted diseases are an important health concern generally, and especially among young men aged 20 to 24, the age group with the highest gonorrhea infection rate. As a result, it is important to understand the influence of different factors such as alcohol in causing the spread of these infections. This study's findings suggest that targeting alcohol use may not be a particularly effective way of reducing STD infection rates. Secondly, the MLDA is an important policy instrument used to influence alcohol consumption among young adults, and it is important to understand its effects on different outcomes. While the increase in drinking at the MLDA of 21 years has been shown to have negative effects on various outcomes such as mortality, crime, and college performance (Carpenter and Dobkin 2009; Carrell, Hoekstra, and West 2011; Carpenter and Dobkin 2015), the results of this study indicate that it does not lead to an increase in sexually transmitted diseases.

### 3. HOUSING VOUCHERS, INCOME SHOCKS, AND CRIME: EVIDENCE FROM A LOTTERY

#### 3.1 Introduction

The U.S. government provided \$16.6 billion in rent subsidies to disadvantaged families through the Housing Choice Voucher Program in 2013 (Center on Budget and Policy Priorities 2014). Historically, the U.S. government provided housing directly to families in the form of housing projects, though there has been a shift in the last few decades toward housing voucher programs. The federally-funded Housing Choice Voucher Program provides rent support to about 2.1 million households living in non-government housing, which is around 43% of all households receiving federal rental assistance (Center on Budget and Policy Priorities 2012; 2013). The program, often simply called “Section 8,” is designed to allow participants to reside in areas otherwise unaffordable and provide an in-kind transfer to low-income families and individuals. The program is means-tested, and participating families receive a rent subsidy that is paid directly to their landlords.

In this paper, we examine the effect of Section 8 vouchers on crime. Vouchers could affect crime through two major channels: income transfer effects and neighborhood effects. Income transfers can relieve financial pressures that could otherwise cause recipients to seek illicit income. Alternatively, income transfers could also provide the funds or leisure time necessary to participate in illegal activities. Voucher receipt could also affect criminal involvement by changing neighborhood influences. Moving to a better

neighborhood could reduce crime via positive peer effects or social norms, or it could increase crime by providing easier and wealthier targets.

Understanding the causal effects of housing mobility programs is challenging because individuals select to participate in these programs. Eligible families that opt to use vouchers may also take other steps to better their lives, creating a substantial source of selection bias. Many studies of voucher programs rely on randomized social experiments, such as the Moving to Opportunity (MTO) experiment. Often, Section 8 housing vouchers are given out via randomized lottery because it is not an entitlement program and there are usually more applicants than vouchers. This random variation in voucher allocation has been relied upon for identification of effects on a host of juvenile outcomes (Jacob, Kapustin, and Ludwig 2015; Jacob, Ludwig, and Miller 2013) as well as adult labor market outcomes.<sup>18</sup>

In this paper, we exploit the exogenous variation in randomized wait-list positions assigned using a lottery in order to identify the causal effects of Section 8 vouchers on arrests of adult household heads. The lottery we study was administered by the Houston Housing Authority (HHA). We link the voucher recipients to arrest records from the Houston Police Department (HPD) to determine whether voucher receipt has an effect on arrests for various types of crimes. We estimate the effects using intent-to-treat models

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<sup>18</sup> Others have used the Gautreaux Program (a precursor of MTO) (Popkin, Rosenbaum, and Meaden 1993), random assignment into public housing (Oreopoulos 2003), or Hurricane Katrina (Hussey, Nikolsko-Rzhevskyy, and Pacurar 2011) to study mobility and crime.



identified using the timing of voucher receipt, which is determined by the randomized lottery.

To support the assumption that wait-list positions are indeed random and that there are no differences between those who lease-up with a voucher (i.e. use a voucher to pay their rent) earlier and those who lease-up later, we perform empirical tests for differences in pre-lottery characteristics of voucher recipients. The relationships between pre-lottery characteristics and wait-list positions are consistent with wait-list randomization, and the types of individuals who lease-up at different times are no different.

Results indicate that some criminal activities actually increase while others remain unchanged due to voucher receipt. We find that the probability of being arrested for a violent offense in a quarter increases by 0.068 percentage points (a 97% increase). Our results highlight an unintended consequence of the Section 8 Housing Voucher Program – an increase in arrests for violent crime.

We attribute this increase to the additional disposable income and leisure time available to voucher recipients that can be used to commit crimes; both of these mechanisms have been shown to increase illegal activity previously (Dobkin and Puller 2007; Riddell and Riddell 2006; Foley 2011; Lin 2008). These effects may be stronger for groups of recipients more susceptible to crime, and we find that the effects are driven by recipients who had been arrested in the past and by males.

Our contribution to the literature is three-fold. The primary contribution is that we are the first to consider the effect of housing vouchers on criminal outcomes for *adult*

recipients using a randomized lottery.<sup>19</sup> We join an extensive crime literature produced by MTO, which, with the exception of Ludwig and Kling (2007) who studied the contagion effects of neighborhood crime on both adults and juveniles, primarily focuses on outcomes for youth whose families received vouchers. While most of these studies have found that MTO caused positive or neutral effects for female youth, their findings for male youth have been surprisingly negative (Clampet-Lundquist et al. 2011; Kling, Ludwig, and Katz 2005; Sciandra et al. 2013; Zuberi 2012). The exception is Katz, Kling, and Liebman (2001), who show that male youth have less behavior problems after their families received vouchers through MTO.

The effect of Section 8 voucher receipt on adult criminal outcomes is yet to be documented although Jacob, Kapustin, and Ludwig (2015) use a lottery-based identification strategy to show that there is no substantial effect on arrest rates of juveniles whose families received vouchers (among other outcomes).

Secondly, we study the impact of residential mobility in the context of the Section 8 voucher program which accounts for a significant portion of federal housing assistance (43% according to the Center on Budget and Policy Priorities 2013). Hence, our results are relevant for a large fraction of US housing aid. Again, we are the first to consider the effects of Section 8 voucher receipt on adult criminal outcomes using a lottery, so the policy implications of our results are significant.

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<sup>19</sup> Leech (2013) uses NLSY data to study the relationship between voucher receipt and self-reported violent altercations for young adult heads of household receiving vouchers. She suggests that selection bias is a methodological shortcoming of her study. She finds that voucher receipt is associated with reduced violent altercations, but that this association is not present in the subsample of black recipients.

Finally, our results speak to the relative impacts of the neighborhood and income effects that arise due to voucher receipt. We provide new evidence that the neighborhoods into which recipients move are only slightly different from their pre-voucher neighborhoods in demographic and economic characteristics. This result is in agreement with existing literature on Section 8 vouchers (Jacob and Ludwig 2012; Lens 2013) and suggests that the effect of the income transfers maybe be the larger influence. We also believe that income transfer effects dominate because the increase in arrests that we detect is in line with the negative outcomes found in the previous literature on government cash transfer programs (Dobkin and Puller 2007; Kenkel, Schmeiser, and Urban 2014; Riddell and Riddell 2006; Evans and Moore 2011; Foley 2011).

Additional income can also affect crime by altering recipients' employment decisions in that it may afford recipients the opportunity to take additional leisure time, which they could use to participate in crime, among other things. Empirically, Section 8 voucher receipt does, in fact, cause lower labor force participation rates and earnings (Jacob and Ludwig 2012; Carlson et al. 2012), and a similar effect has been detected for Food Stamps (Hoynes and Schanzenbach 2012).

Overall, our study documents an unintended consequence of Section 8 housing vouchers (an increase in arrests for violent crime). The program is the largest housing assistance program in the U.S., so this repercussion could be quite large on a national scale. The disparity across subgroups implies that large income shocks have heterogeneous effects on recipients and has policy implications for screening and oversight within the voucher program.

### 3.2 Background

The Section 8 Housing Voucher Program is the largest housing assistance program in the U.S. The vouchers are federally-funded, and the U.S. Department of Housing and Urban Development (HUD) allocates the funds to local housing authorities and sets eligibility standards across the nation. HUD requires that participants' incomes fall below 80% of the median family income in the area, adjusting for family size, and stipulates that 75% of new voucher recipients' incomes are less than 30% of the local median family income (Center on Budget and Policy Priorities, 2013). Voucher recipients must also be citizens or of other eligible immigration status, and local housing authorities can deny eligibility for a history of criminal activity (HUD 2001; HHA 2013). Local housing authorities submit the subsidies directly to the recipients' new landlords. Continued eligibility is assessed annually, and recipients are allowed to use their vouchers in any U.S. city with the Housing Choice Voucher Program in place, although, according to HHA, less than 10% of their voucher recipients move to a different city.

The Houston Housing Authority (HHA) serves around 60,000 Houstonians, over 80% of whom are participants in the Housing Choice Voucher Program. HHA accepted voucher applications from December 11, 2006, to December 27, 2006, and received over 29,000 applications. All applicants were assigned a lottery number regardless of whether they met the eligibility criteria. Vouchers were then extended to the applicants as the funding became available starting with the lowest lottery numbers. The lottery and voucher service processes are outlined in Figure B.1. Once an applicant's wait-list position

was reached, he or she received a voucher screening packet from HHA and the verification process began. After their eligibility was verified, families were required to sign a lease in a Section 8 approved community in order to participate in the program. The average time between HHA sending the initial packet and the recipient leasing up with the voucher was 6 months. Because the speed of this process varied by applicant, the vouchers were not issued in perfect sequential order.<sup>20</sup>

The first vouchers were issued in July 2007. However, the majority of vouchers were serviced starting in 2009, and HHA had sent screening packets to almost all the lottery numbers by October 2012. Overall, the take-up rate was about 23%. The low take up is a result of applicants dropping out at every step of the voucher service process. Based on the last known application statuses, close to 60% of the verification packets were not returned to HHA by the families. 2.5% of the applicants were found to be ineligible after verification and about 4% of them were unable to sign a lease in time, and the voucher expired.

We geocode the addresses provided on the applications and the addresses of current residents (in 2014, when the data were obtained) in order to describe the pre- and post-lottery neighborhoods of voucher recipients. Figure B.2 shows the density of these two types of addresses across the city using heat maps and contains the boundaries of HPD's

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<sup>20</sup> In addition, some lottery numbers were called too far out of order for this to be the case. HHA says that there were no priority groups in the lottery, and there are no common characteristics of these applicants who were called out of sequence. However, because we use the assigned lottery number to predict voucher service, our estimates should not be biased by the occasional non-sequential servicing of lottery numbers.

police beats.<sup>21</sup> The distribution of addresses indicates that the voucher-users are not moving to different parts of the city on the whole.

Changes in neighborhood (defined as Census Tract and police division) experienced by the voucher recipients are documented in Table B.1. Around 14% of voucher recipients did not move and instead used the voucher at their address at the time of application; nearly 30% stayed in the same Census Tract. The median distance moved is 3.01 miles, and the voucher paid an average of \$628 toward rent every month. Only 3.4% of these recipients were living in public housing at the time of application. Differences between the neighborhoods before and after the lottery are described in Panel B. We report median rent in 2012 from the American Community Survey, and we see that voucher recipients lease-up in census tracts with only \$39 higher monthly median rent. We report demographics and socioeconomic characteristics of the census tracts from the 2010 census and crime rates from 2000 to 2005 for the police divisions. The post-lottery neighborhoods are somewhat better off in terms of quality parameters such as unemployment rate, household income, poverty rate and crime rates.

These differences in neighborhoods are minimal; for example, voucher-use neighborhoods had on average 2.1 less crimes per year per 1000 residents than application neighborhoods, which is a 1.5% improvement. As a result, we believe that any impact of the vouchers in this context can be most reasonably attributed to the income shock induced by an annual rent subsidy of more than \$7,500 on average. Moreover, if we assume that

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<sup>21</sup> The heat maps are created in ArcMap using a point density operation that creates a grid over the map and then counts the number of address points within each grid cell.

voucher recipients were paying the median rent in their Census Tracts of residence before voucher receipt (\$797), because they contribute on average \$205 to rent once they receive a voucher, they are paying \$592 less on housing per month.<sup>22</sup> To voucher recipients, these newly-available funds are no different in effect from a direct cash transfer. Conversely, the difference in the average median rent between pre- and post-voucher Census Tracts is only \$39, indicating that the majority of the voucher does not go towards improved housing but instead impacts recipients like a cash transfer.

Additional income can be spent on things that can increase or decrease the likelihood of arrest. It could also alleviate financial pressures, which would reduce the recipients' motivations to be involved in crime that can lead to financial gain, such as selling illegal drugs or theft. The net effect is ambiguous, and the question will ultimately have to be answered empirically. The theoretical implications of an in-kind transfer on labor decisions are similarly ambiguous because they depend on the shape of each recipient's indifference curves. However, researchers find that vouchers reduce earnings and labor force participation (Jacob and Ludwig 2012). Like additional income, additional leisure time can be put toward things that either increase or decrease the likelihood of arrest.

Given that much of the existing literature has examined MTO, it is important to highlight the differences between MTO and the housing voucher program. MTO researchers recruited only public housing residents to participate and split them into three groups. The first (the "MTO experimental group") received vouchers and was only

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<sup>22</sup> We consider this estimate to be an upper bound of the effective cash transfer because voucher recipients may have paid rents below the median rents in their Census Tracts before receiving a voucher.

allowed to use them in Census Tracts with low poverty rates. The second group was simply given vouchers that could be used anywhere without restrictions. This group was called the “Section 8 experimental group” because their treatment was similar to Section 8. The third was a control. The neighborhoods into which MTO experimental families moved were notably different from the ones that they left (Katz, Kling, and Liebman 2001; Kling, Ludwig, and Katz 2005). The MTO Section 8 experimental group moved to areas more like their neighborhoods of origin than the MTO experimental group (Kling, Ludwig, and Katz 2005), although there was some improvement. Similar to findings for the MTO Section 8 group and Jacob and Ludwig’s findings (2012), we find that Census Tract characteristics of new neighborhoods are slightly improved, but the changes are not large. Additionally, the neighborhood changes we detect are smaller in relative terms than those found in MTO studies for the MTO experimental group. For example, HHA voucher recipients moved to Census Tracts with a 7.5% lower average poverty rate, while MTO experimental group participants moved to census tracts with a 27% lower average poverty rate (Kling, Liebman, and Katz 2007).

MTO’s driving mechanisms were also different because it targeted families living in public housing, and therefore already receiving housing assistance. MTO required the families to move and provided little, if any, additional financial gains to them. Section 8, on the other hand, provides a substantial income transfer, and HUD does not allow local housing authorities to place restrictions on neighborhoods in which recipients can use vouchers. While we don’t have information on the Section 8 participants’ reasons for applying for the program, it is well documented that MTO families cite a desire to get



away from gangs and drugs as the main reason for volunteering (e.g. Kling, Ludwig, and Katz 2005). This concern is likely addressed by the neighborhood change facilitated by MTO, but Section 8 voucher receipt may have little effect on this. The populations opting into these two programs are also likely to be quite different due to incongruous motivations.

### 3.3 Data

The Houston Housing Authority provided us with information on the voucher applicants. These confidential data include lottery numbers, the number of bedrooms needed (calculated based on family size), the date on which HHA sent the voucher screening packet, and the move-in date for voucher recipients. The data also include name and birthdate, which we use to match the HHA data to arrest records. They also provided additional, more detailed information on the set of applicants who were participants in the Housing Choice Voucher Program in 2014. For this group, we also know their race and homeless status at the time of admission, as well as their voucher-use address.

HHA assigned lottery numbers up to 29,327, but we limit our sample to those living in Houston at the time of the lottery. Additionally, there are a small number of duplicate applicants; we assign them their lowest lottery number. We also drop applicants with lottery numbers over 24,000 because the take up rate is much lower among the later lottery numbers indicating a probable change in the voucher service process after that point.

Additionally, we restrict our analysis to those applicants who eventually leased-up with a voucher. Estimates from the sample unconditional on take-up are of similar magnitudes as those from the sample conditional on take-up, but are measured imprecisely

given the relatively low take-up rates in Houston. The take-up rate is only 23%, which is low relative to the 69% national average estimated by Finkel, Pistilli, and Buron (2001). We also perform empirical tests, detailed in the following section, to support the assumption that the population of takers with low lottery numbers is no different from the takers with high lottery numbers. The resulting sample size is 4,510.

Table B.2 reports pre-lottery descriptive statistics. We report them for the population of voucher-users, and we show them separately by low and high lottery numbers (applicants with lottery numbers below and above the median) to demonstrate the similarity between applicants (prior to the lottery) whose vouchers were serviced early and those whose vouchers were serviced later. If these groups are different on important measures, it could indicate that HHA gave preference to some groups in lottery number assignment or that the type of individual who leased-up with a voucher changed over time.

The first panel of Table B.2 pertains to the lottery implementation. The means of lottery numbers in the two groups differ by about 11,000. In the analysis that follows, treatment is leasing-up using a voucher. Intuitively, the “voucher service” quarter (intent-to-treat) is the quarter during which the applicant would have leased-up according to lottery number. On average, recipients take approximately 6 months to complete the screening process and actually relocate using the voucher. We determine whether the individual has been sent a screening packet by a given quarter based on his or her lottery number relative to the numbers called by that point.<sup>23</sup> Lagging this by two quarters gives

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<sup>23</sup> Since the lottery numbers were not called in perfect sequential order, we cannot identify the range of lottery numbers simply using the smallest and largest lottery number called in a quarter. Additionally, for

us the “voucher service” quarter. The low lottery numbers were serviced about 1.5 years (5.8 quarters) before the high lottery numbers on average.

The average voucher recipient was around 35 years old at the time of the lottery and required just over two bedrooms (indicating that the average family size was between 2 and 6, Housing Choice Voucher Program Guidebook (2001). Around 94% of recipients are black, and using 2012 voting records from the Harris County Tax Assessor’s office, we estimate that nearly 90% of applicants are female.<sup>24</sup> Less than 1% of recipients were homeless at the time of admission to the program. The number of observations varies for race and homeless status because they are only available for current (2014) HHA voucher recipients. There is only one statistically significant difference between the high and low lottery numbers on any of these measures (number of bedrooms required), and it is not economically significant.

We match the HHA data to arrest records provided by the Houston Police Department (HPD). The arrest records are reported at the time of booking and include information on the offense as well as the arrestee’s name, birthdate and reported home address. We match the HHA and HPD data using name and birthdate, and we perform secondary matches

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approximately 5,000 applicants, there is no recorded screening packet issue date. As a workaround, within each quarter from 2007 to 2011, we take the lottery number at the 75th percentile of the numbers called in that quarter to be the last number called in that quarter. We assign the next lottery number as the first number called in the subsequent quarter.

<sup>24</sup> We calculate the percentage of Harris County voters whose reported gender is “male” for each unique first name in the list of registered voters. If there are at least 5 individuals with a given name, and 70% or more are listed as males, the name is assigned the gender “male.” If 30% or less are listed as male, we classify the name as “female.” Applicants with unmatched or ambiguous names are omitted from the gender subgroup analysis.

using the Levenshtein distance and soundex code of each name for unmatched records.<sup>25</sup> The arrest records range from January 1990 to November 2011.<sup>26</sup> We also use the matched arrest records to create measures of criminal activity in the period before the lottery and a quarterly panel of arrests for the study period after the program commenced (from quarter 1 of 2007 to quarter 3 of 2011).

We consider arrests of any type and specifically categorize violent offenses, drug offenses and financially-motivated offenses. We measure arrests as a binary indicator for whether the recipient was arrested. The pre-lottery crime measures are constructed for the 5 years prior to the lottery, and we create an additional binary indicator for whether the applicant was arrested at least once between 1990 and 2006. Around 20% of applicants were arrested during that 16 year period, and approximately 9% of applicants had been arrested in the 5 years prior to the lottery.<sup>27</sup> There are no statistically significant differences between high and low lottery number individuals.

Using the geocoded application addresses, we find that voucher recipients lived in Census Tracts with around 51% black residents, and around 36% Hispanic residents. The mean unemployment rate was around 12% and the mean of median family income was approximately \$34,000. The mean poverty rate was quite high at over 30%. Voucher

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<sup>25</sup> For the arrest records that are unmatched by name and birthdate, we calculate the Levenshtein distance for the first and last names, if the sum of the Levenshtein distances is less than 3, conditional on an exact birthdate match, we accept the match. For the records that are still unmatched, we perform an exact soundex code match.

<sup>26</sup> The Houston Police Department has denied our requests for additional data, so we are not able to extend the panel further into the post-lottery period.

<sup>27</sup> HHA performs criminal background checks on all adult family members to ensure that they have “no drug-related or violent criminal history during the past 5 years” (p. 18, HHA 2013). HHA obtains conviction records, so any potential leasers who were arrested but not convicted would be eligible.

recipients with higher lottery numbers lived in census tracts with slightly higher unemployment rates and slightly lower poverty rates. Voucher recipients lived in police divisions with an annual average of 135 crimes per 1000 residents. On average, nearly 60 of these crimes were property crimes and only 13 were violent. Recipients with higher lottery numbers lived in neighborhoods with 1.1 more crimes per year per 1000 residents, a marginal difference considering the average crime rate. Although some of these differences are statistically significant, none of them are economically significant. The similarity between these groups indicates that pre-lottery characteristics are distributed randomly across lottery numbers and suggests that the lottery was in fact random.

In Table B.3, we report post-lottery descriptive statistics. The purpose of this table is to preview results in a cross-sectional manner. We show measures of program take-up (whether the individual's voucher has been serviced and whether he or she has leased-up by a quarter) as well as all of the arrest outcomes averaged over person-quarters (from quarter 1 of 2010 to quarter 3 of 2011). Statistics are restricted to last 7 quarters of the panel, when vouchers of individuals with low lottery numbers had mostly been serviced, but individuals with high lottery numbers had not had their vouchers serviced. Specifically, vouchers of individuals with lottery numbers below the median had been serviced, on average, for 89% of person-quarters. Conversely, the vouchers of those with lottery numbers above the median had been serviced for around 17% of person-quarters during this period. Lease-up follows a similar pattern where individuals with low lottery numbers are nearly 70 percentage points more likely to have leased up during a person-quarter. The post-lottery statistics for the outcomes – probability of arrest in a person-

quarter for different crime categories – indicate that recipients with low lottery numbers are considerably more likely to be arrested for crimes of any type and for violent crimes in this period.

### 3.4 Identification and Methods

In this study, we identify the effect of housing vouchers on criminal involvement using a lottery. The lottery randomized the order of the wait-list from which applicants were called for voucher service and, therefore, the order of actual voucher receipt. This randomization allows us to identify the causal effects of voucher receipt. Because the random variation we exploit for identification is in timing, we analyze criminal outcomes using a quarterly panel of arrests using pooled cross-sectional models.

Because we consider the group of applicants who eventually lease-up with a voucher, our identifying assumption is that the timing of voucher receipt among those who eventually received the voucher was exogenous. That is, we assume that within the group of participants who lease-up using a voucher, the low lottery number individuals (who leased up earlier) had similar propensities to commit crime as those with higher lottery numbers (who leased up later). We condition on lease-up because the take-up rate is particularly low for this lottery, resulting in imprecise estimates for the entire sample. Take-up rates are consistent across time, which we will show in Section 5. For this reason, we believe that the leasers with low and high lottery numbers are no different, and we show results from additional empirical tests to support this in the following section.

Before we estimate intent-to-treat effects of the vouchers, we first examine evidence on whether the randomization was properly implemented and whether the leasers with low

lottery numbers are different from those with high lottery numbers. We test this empirically by examining the extent to which demographic and criminal history variables are correlated with lottery number or voucher service quarter. We represent this graphically by simply plotting these characteristics against lottery number and estimate it empirically according to the following equation:

$$control_i = \alpha + \beta \text{ voucher order}_i + u_i \quad (2.1)$$

In the above equation, *voucher order<sub>i</sub>* is either the randomized lottery number assigned to applicant *i* or his/her voucher service quarter (where the first quarter of 2007 is indexed to one). We test each applicant's age at the time of lottery, number of bedrooms, and the set of criminal history variables: whether (and how many times) the applicant was arrested in the 5 years prior for any type of offense, a violent offense, a drug offense, or a financially-motivated offense, and whether the applicant was ever arrested between 1990 and 2006. We also look for correlations in race and homelessness status at time of admission (for 2014 residents), neighborhood characteristics prior to the lottery (for the applicants whose addresses were geocoded successfully), and gender (for those whose gender we could impute from their first name as described in Section 3).

To estimate the impact of Section 8 vouchers on arrests, we estimate the intent-to-treat effect of voucher service. We estimate regressions of the following form:

$$outcome_{it} = \rho + \pi \text{ post voucher service}_{it} + \Psi X_i + \phi_t + \varepsilon_{it} \quad (2.2)$$

where *post voucher service<sub>it</sub>* is a dummy variable equal to one if individual *i*'s voucher has been serviced by quarter *t*. The results should be interpreted as the effects of potential voucher use based on lottery number, and can be reweighted by the first stage to recover

a local average treatment effect. To estimate this first stage, we use an indicator for whether individual  $i$  had leased up using a voucher by quarter  $t$ , called *post lease-up* $_{it}$ , as the outcome variable.

We estimate the intent-to-treat effects using a number of arrest outcomes: whether an individual was arrested for crimes of any type, violent crimes, financially-motivated crimes, and drug crimes in quarter  $t$ .

We estimate all models using quarter fixed effects ( $\varphi_t$ ) as well as robust standard errors that are clustered at the individual level. All specifications are estimated both with and without controls ( $X_i$ ) for past crime (probability of arrest for the particular crime category in the 5 years prior to the lottery), age at the time of the lottery and a proxy for family size (number of bedrooms); this tests whether timing of voucher service is correlated with any of the observable characteristics.<sup>28</sup> If specifications that do and do not include controls yield similar estimates, this can be interpreted as evidence that is consistent with randomization of timing of lease-up. We also replicate the main results using a negative binomial model to show that results are not sensitive to the parametric specification imposed by the linear probability model.

We take a cue from the existing mobility literature and explore the possibility of dynamic effects over time (Kling, Ludwig, and Katz 2005). Specifically, we estimate separate treatment effects for the first year after voucher service and later years of voucher service by using two binary treatment variables. The first is equal to one if the applicant's

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<sup>28</sup> We perform additional analyses controlling for application address Census Tract characteristics and police division crime statistics in Table B.11 because they are not available for all recipients.



voucher had been serviced within the past year, and the second is equal to one if the applicant's voucher had been serviced more than a year ago.

In order to further explore potential mechanisms and policy implications, we replicate our main analysis for 2 pairs of subgroups. We compare results for recipients with and without past arrests because we believe that past arrests may signal a propensity for crime. We then separate out males and females because men have much higher arrest rates for violent crime than women.

### 3.5 Results

#### *3.5.1 Tests of Identifying Assumption*

Identification of the model comes from the assumption that the timing of voucher receipt among those who eventually received the voucher was exogenous. That is, we assume that within the population of leasers, individuals with lower lottery numbers had similar propensities to commit crime as those with higher lottery numbers. Because the timing of voucher packet issue and therefore subsequent transition into subsidized housing was determined by a randomized lottery, this is a reasonable assumption. Nevertheless, we test this assumption empirically in several ways.

First, we test this by showing that take-up rates did not change over time. If the rate had changed as HHA serviced higher lottery numbers, it could indicate that within the population of leasers, those with high lottery numbers may be different from those with low lottery numbers. Figure B.3 plots take-up rates over lottery numbers. Take-up rates do not appear to change over the range of lottery numbers. We also test this empirically to determine whether there is a correlation between lottery number and take-up. We report

estimates of this correlation within the figure, and there is not a statistically significant relationship.

Second, we test for correlations between observable characteristics and both lottery number and voucher service quarter. If the identifying assumption holds, we expect to see no correlations between these measures and demographic variables or criminal history measures. For example, if the most motivated applicants were assigned lower numbers through manipulation of the lottery mechanism, we would see a negative correlation between lottery number and indicators of stability such as age, gender, and criminal history. Alternatively, within the group of high lottery numbers, if only the most stable individuals lease-up (because they are more likely to stay at the same address for an extended period, thereby remaining reachable by HHA), we would see a positive correlation.

Figure B.4 represents these relationships graphically for criminal history (probability of past arrests, past violent arrests, past drug arrests and past financial arrests) and demographic (age and number of bedrooms) variables. Each dot is a local average for a bin of 250 lottery numbers. If lottery number is truly random and the leaser population is constant over time in observable characteristics, the local averages should exhibit a flat relationship. This does appear to be the case, and we take this as support for the identification assumption.

Table B.4 reports the results of the empirical tests. Column 1 contains the results from 24 separate regressions using lottery number as the independent variable as described by equation 2.1. Similarly, the regressions that generated column 2 all use indexed voucher

service quarter as the independent variable. Each row is labeled for the covariate used as the dependent variable.

There is only one statistically significant correlation between individual characteristics and voucher order. This effect is on the number of bedrooms, but it is not economically significant. It predicts that the individual with the highest lottery number (24,000) would require 0.11 more bedrooms than the individual with the lowest lottery number. There are no significant relationships between lottery number or voucher service quarter and criminal history measures (perhaps the most important determinants of future arrests).

There are a few significant correlations between voucher order and neighborhood characteristics, but none of them are economically significant. The leasers with higher lottery numbers come from Census Tracts with higher unemployment rates and lower poverty rates. They also come from police divisions with higher crime rates overall and for violent crimes. Again, none of these differences are economically significant. For example, if we consider 2 applicants whose vouchers were serviced 2 years apart, we would expect the later-served applicant's original neighborhood to only have 3.25 (2% of the mean) additional crimes per 1000 population annually. Importantly, because we find an increase in violent crime arrests for recipients, if we assume recipients from low crime neighborhoods have a lower propensity for crime, any indication that leasers with lower lottery numbers came from better neighborhoods would imply that our findings are a lower bound of the true increase. As an additional check, we also estimate the main models with and without these controls and show that the results are invariant, indicating that timing of voucher service is orthogonal to these characteristics.

### 3.5.2 *Effect of Voucher Service on Lease-Up*

Before examining the effect of voucher receipt on criminal outcomes, we first document that the voucher recipients are likely to lease-up when we predict that their vouchers were serviced. Our ability to use lottery variation to identify effects hinges on the extent to which the lottery predicts lease-up.

Table B.5 contains the first stage results obtained by estimating equation 2.2 using *post lease-up* as the outcome. The table reports the coefficient on *post voucher service* from 2 separate regressions. The results indicate that in 84.9% of the person-quarters after voucher service, the voucher recipient had previously leased-up. This coefficient is identical when we include controls in column 2, suggesting that controls are orthogonal to *post voucher service*. The large magnitude of the first stage results means that the intent-to-treat estimates will be very close to the local average treatment effects.

### 3.5.3 *Effect of Voucher Service on Arrests*

Table B.6 contains the main results for the full sample of voucher recipients. We estimate equation 2.2 to measure the intent-to-treat using both ordinary least squares and a negative binomial model. We also report the mean of each outcome variable from the year preceding the lottery (2006) for the relevant population; we refer to it as the “pre-lottery mean.” Each panel is labeled for the outcome variable for which the results are generated. We run models both with and without controls and demonstrate that our results are unresponsive to their inclusion, indicating that the timing of voucher service is

unrelated to these observable characteristics and, we expect, to the unobservable characteristics.<sup>29</sup>

Results show no evidence that voucher service and lease-up affect arrests for all types of crimes combined. All of the coefficients are statistically insignificant. We also look at arrests for specific types of crimes that are likely to be affected by voucher receipt: violent crimes, drug crimes, and financially-motivated crimes. We find statistically significant effects on violent crimes. The magnitude of said effect indicates that voucher receipt increases quarterly probability of violent crime arrest by 0.068 percentage points. Comparing this estimate to the mean pre-lottery quarterly probability of violent crime arrest (from 2006), it represents a 97% increase. In absolute terms, these results suggest an increase of 2.7 violent crime arrests per 1000 recipients annually. The neighborhoods into which the recipients move have on average 13.2 reported violent crimes per 1000 residents annually. If each reported violent crime results in one arrest on average, this increase may be associated with an approximately 21% increase in neighborhood crime.

Negative binomial results for violent crime are similarly large and statistically significant. Results indicate around a 78% increase in violent crime arrests. We also find evidence that recipients are arrested for more violent crimes in the 6 months during which their eligibility verification and voucher process is underway but they have not yet leased-up (Table B.10). This increase is the effect of an impending income shock and can be

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<sup>29</sup> Table B.6 contains models that include controls observed for the entire sample. We also rerun the main models adding neighborhood controls only available for a subset of recipients. Results are not statistically different from those here, the effect on violent crimes remains statistically significant (the coefficient is 0.000693 compared to 0.000676) and coefficients change minimally between models with and without controls. Results are in Table B.11.

interpreted as an announcement effect.

Drug crime arrests appear to be unaffected by voucher receipt. Effects are statistically indistinguishable from zero. Financially-motivated crime arrests also appear to be unaffected by voucher receipt. The coefficients are positive and large, but are not statistically distinguishable from zero. We attribute the lack of significance to limited statistical power given the small sample size.

As discussed earlier, one might also expect differential effects by how long an individual has been treated (as Kling, Ludwig, and Katz 2005, found for juveniles). Table B.7 contains the results from models that allow for the effect of voucher service to vary over time. Specifically, we estimate effects of two different intent-to-treat measures: whether the applicant's voucher was serviced within the last year, and whether the applicant's voucher was serviced more than a year ago. Because the bulk of vouchers were serviced in 2009 or later and our panel ends in 2011, most applicants were treated for just over 2 years or less. Because ordinary least squares results and negative binomial results are so similar for the main results, we estimate these models using just ordinary least squares for simplicity.

Panels A to D contain results from different crime categories. Similar to results reported previously, there is little evidence of an overall effect for all arrests, drug arrests and financially-motivated arrests. Violent arrests are slightly more responsive to voucher receipt during the first year of voucher use, although the coefficients for the first year and later years are not statistically different from each other. In summary, we find that voucher

receipt causes a rather large increase in violent crime arrests for recipients. We find that the vouchers have no effect on other types of crime.

#### 3.5.4 *Subgroup Analysis*

There are a number of reasons to expect some types of individuals to respond differently to the vouchers. In this section, we test numerous hypotheses about the cause of this increase in violent arrests and narrow in on a plausible explanation.<sup>30</sup> It is reasonable to postulate that if the voucher makes individuals more likely to commit a crime, those who have a higher propensity for crime will respond more strongly. We compare recipients who have been arrested in the past to those who have not because they have demonstrated such a propensity for crime. Then, we compare males to females because males are more likely to be arrested in general and in our sample. Additionally, MTO studies have consistently found asymmetric effects by gender (Katz, Kling, and Liebman 2001; Clampet-Lundquist et al. 2011; Jacob, Kapustin, and Ludwig 2015; Ludwig and Kling 2007; Kling, Ludwig, and Katz 2005; Kling, Liebman, and Katz 2007).

Table B.8 contains results for the subgroups. The first 2 columns compare results for recipients with (column 1) and without (column 2) any past arrests. As in the full sample, the result for violent crime is large and statistically significant, but only for the recipients with at least one past arrest. The effect represents an increase of nearly 70% compared to the pre-lottery mean for this subgroup. The coefficient for recipients without a previous

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<sup>30</sup> We test 2 other pairs of subgroups and include those results in Table B.13: younger vs. older recipients (under and over 30 years old) and “non-movers” (who use their voucher at their application addresses) vs. “movers.” There are no results of note for the age-related subgroups. Non-movers are more likely to be arrested for any type of crime than movers, but because the decision to move is endogenous, we are unable to disentangle the difference between being a non-mover “type” and the effects of not moving.

arrest is also positive, but it is small and not statistically significant. For the previously-arrested sample, there is a sizable positive effect on financially-motivated arrests, but the coefficient is not statistically different from zero.

In the second set of columns (3 and 4) we compare results by gender. We are only able to perform this analysis for recipients whose gender we could impute by their first name as described in Section 3, so the number of individuals is less than that used for the main analysis. We find that males are in fact more likely to be arrested for a violent crime than the females due to the voucher. The coefficient for violent crime for males is large, positive, and statistically significant, while that for females is small, negative and not statistically significant.

If male-headed households are more likely to have multiple adults, voucher receipt could increase partner domestic violence either by changing the domestic balance of power in families or by allowing for increased consumption of alcohol and drugs. The arrest records from the Houston Police Department do not identify domestic violence as a particular type of offense, but because we observe both home and arrest addresses, we can consider violent crimes occurring at home as a proxy for reported domestic violence. Only 14% of violent crimes committed by these males occur at home, so these offenses are not driving our results.

### *3.5.5 Test for Attrition*

One potential concern for our study is attrition. That is, to the extent that individuals with low lottery numbers are more or less likely to move out of Houston than individuals with high numbers, our results could be biased. For example, if individuals who receive



high lottery numbers are more likely to leave Houston and commit crimes elsewhere that are not measured in our data, then our results could overstate the increase in violent crime due to housing vouchers.

We empirically test whether applicants with lower lottery numbers and earlier voucher service quarters are more or less likely to have stayed in Houston than those with higher numbers and later voucher service quarters. We proxy for continued Houston residence with whether the applicant was registered to vote in the City of Houston in 2012 and whether he or she voted in the 2012 general election. Specifically, we estimate an analog of equation 2.1 used in the test of randomization, to test for a relationship between when an applicant's voucher was serviced and whether he or she stayed in the city.

We show the raw data in Figure B.5; it plots voter registration and actual voting in 2012 against lottery numbers. Each dot represents a local average for a bin of about 250 lottery numbers. There is no discernable correlation between lottery number and either voting outcome. This suggests that individuals whose numbers were called early in the sample period were no more or less likely to be in Houston several years later than those whose numbers were called late in the sample period.

Table B.9 contains the results of the empirical test. In column 1 the dependent variable is a binary indicator for being registered in 2012, and in column 2 it is a binary indicator for voting in 2012. There are no significant correlations between when an applicant was

served by HHA (measured by lottery number and voucher service quarter) and the two proxies for Houston residence.<sup>31</sup>

### 3.6 Conclusion

In this study, we analyze whether receiving a housing voucher affects criminal activity of low income individuals. The timing of voucher receipt was determined by an individual's position on the wait-list, which was assigned using a randomized lottery. We use the lottery numbers to determine by when an individual's wait-list number was serviced and estimate intent-to-treat models to determine the effect on arrests.

Results indicate that voucher receipt causes a large increase in violent crime arrests. Over 90% of these arrests are for assaults, and most of those are simple assaults resulting in no bodily injury. We find that if 1000 individuals receive vouchers, we can expect at least 2.7 additional violent crime arrests a year. HHA issued vouchers to 4510 individuals, so they should observe at least 12.2 additional arrests per year. Using an estimated social cost of \$9,971 per assault (Lochner and Moretti 2004), the social cost of 12.2 additional assaults (the least costly and most common type of arrest in our dataset) is \$120,938.21 annually. To the extent that the arrests we observe are only a portion of the underlying crimes, this cost estimate is a lower bound. Nationally, there are 2.1 million Housing Choice Voucher recipients, so these effects could translate to 5,670 more arrests annually, costing over \$56 million across the US.

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<sup>31</sup> Table B.12 contains an additional test. We replicate the main results using only the population who were registered to vote in 2012. Point estimates are 0.000647, compared to 0.000676 in Table B.6, but the coefficient is no longer statistically significant likely due to the reduced sample size.

Recently, long-run studies of the Moving to Opportunity Experiment as well as random moves precipitated by public housing demolitions have emphasized the positive later life impacts of moving to better neighborhoods for children (Chetty, Hendren, and Katz 2016). Although the Housing Choice Voucher Program was designed to facilitate such mobility in addition to providing an in-kind transfer to low-income individuals, our results indicate that the transfer may be the more dominant effect and could be leading to this increase in violent crime arrests. We show that the neighborhoods into which recipients move are only slightly less disadvantaged than their original neighborhoods, which is consistent with previous research (Lens 2013). We also calculate that the effective cash transfer experienced by the recipients is nearly \$600 per month based on the difference between an estimate of their pre-voucher rent expenditures and their actual post-voucher rent contributions.

Based on the relative size of neighborhood and income effects, we believe that individuals in our sample may be spending the extra income on things that lead to violent crime such as weapons, drugs, and alcohol, which is a well-supported outcome in the government transfer literature (Dobkin and Puller 2007, and Riddell and Riddell 2005). Because Jacob and Ludwig (2012) show that Section 8 voucher recipients work less hours, we also believe that additional leisure time contributes to this negative consequence as it affords recipients more time to socialize. If that socialization also includes drugs and alcohol, this is even more likely to be the case.

We find that subgroups that are highly likely to respond such an income shock are driving the increase in violent crime arrests: males and individuals who had been arrested

at least once before receiving a voucher. Both groups are more likely to have ties to criminal gangs, facilitating criminal use of these new resources. Past arrestees may also have difficulty obtaining jobs due to past criminal convictions, leaving them more leisure time.

The most striking and actionable result is that the recipients who had been arrested before receiving a voucher were more likely to be arrested for a violent crime due to receiving a voucher. The Department of Housing and Urban Development empowers local housing authorities to screen recipients based on past criminal history (HUD 2001), and the Houston Housing Authority does so in practice (HHA 2013). Voucher eligibility rules are focused on certain types of more serious crimes committed recently. (In Houston, applicants can be denied for a drug or violent crime in the past 5 years.) The policy implications of this result are simple and clear – these criteria may be too lax and housing authorities may be able to reduce this significant unintended consequence by further restricting eligibility on the basis of criminal history.

## 4. CAN DOMESTIC VIOLENCE VICTIM SCREENING AND ACTIVE SAFETY COUNSELING SAVE LIVES?

### 4.1 Introduction

Although the rate of intimate partner violence in the United States has declined significantly over the last two decades (National Crime Victimization Survey, BJS, 2013), domestic violence still remains a serious social and public health problem. According to a 2010 report based on the National Intimate Partner and Sexual Violence Survey by the Centers for Disease Control and Prevention (CDC 2010), one in three women in the US become victims of intimate partner violence during their lifetime. This abuse could escalate in many cases resulting in serious injuries and sometimes even death. While the overall rate of homicide is greater among men than among women, a far greater proportion of women are killed by their intimate partners than men. At least 35% of female homicide victims, over 1000 women every year, are identified as having been killed by their intimate partner (Supplementary Homicide Reports, 2000-11).<sup>32</sup>

One strategy that policy makers have used to address this problem is to deter or incapacitate the offender through harsher penalties on acts of domestic violence. In this light, many states passed mandatory arrest laws that mandated warrantless arrests of suspects for every a domestic violence incident reported. However, Iyengar (2009) finds

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<sup>32</sup> Based on Supplementary Homicide Reports from 2000 to 2011, in about 35% of the female homicide cases, the victim is identified as the girlfriend or wife of the offender. In as many as 25% of the female homicide cases the relationship between the victim and offender is not known and in about 17% of the cases the victim is identified as an acquaintance or a friend of the offender. It is possible that many homicides in these groups could also be results of intimate partner violence.

that these laws had precisely the opposite effect, increasing intimate partner homicide among married couples by 60%. The author suggests that this increase is mostly a result of reduced reporting of domestic abuse by victims fearing the certain arrest of their partner and increased retribution from the abuser after the arrest.

In this paper, I study a program that takes an alternate strategy of identifying and empowering the most high danger victims with the right resources in order to reduce intimate partner homicide. The Lethality Assessment Program (LAP) was developed by the Maryland Network Against Domestic Violence (MNADV) in 2005 and was thereafter implemented by all the law enforcement agencies in Maryland. The model provides police officers an objective scale to assess the risk after a domestic violence incident and identify the most high-danger victims based on their responses to the lethality assessment questionnaire. Once the victim is identified as having a high-risk of serious injury or death, the officer contacts a local domestic violence hotline, gets input from a counselor on the victim's situation, and conveys a tailored safety plan to the victim, while encouraging her to speak with the counselor at every step. According to the statistics from MNADV from 2006 to 2009, nearly 59% of the female victims (70 out of 100,000 population) who were screened as high risk chose to speak with a counselor and at least 17% (20 out of 100,000 population) sought help at the shelter at a later time.

While many instruments are available for different types of domestic violence risk assessment, none of them have been evaluated using experimental or quasi-experimental

techniques.<sup>33, 34</sup> In the course of 11 years since it was piloted in 2005, the Lethality Screen instrument has garnered a lot attention, and the LAP has been implemented by numerous law enforcement agencies across 34 states. Recently, the Lethality Screen instrument has been studied in greater detail. Robinson, Pinchevsky, and Guthrie (2016) have studied the extent to which police officers agree with the importance of risk factors highlighted in the Lethality Screen in assessing the level of risk, and they find that the officers are generally in agreement. Through follow-up surveys of high danger victims before and after the implementation of LAP in select jurisdictions in Oklahoma, Messing et al. (2015) find that women who received the LAP intervention were significantly more likely to use protective services and less likely to be victims of physical violence than the comparison group that only received the standard police response. In a follow up study (Messing et al. 2016) the authors explore the variation in victim and law-enforcement agency characteristics and the associated variation in the utilization of services.

The main challenge in addressing the question of the effectiveness of the LAP is that agencies that adopt the program choose to do so, and they might be different in observable

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<sup>33</sup> Different instruments are designed to serve different purposes, and they also differ in their methodologies and ease of use. MOSAIC-20 (De Becker and Associates) and Danger Assessment (Campbell et al. 2003; Campbell et al. 2007; Campbell 1994; Campbell 2001), on which the LAP is based, are some of the instruments that were developed to predict lethality or near lethality in domestic violence situations. Other instruments, such as SARA (Spousal Abuse Risk Assessment) (Kropp and Hart 2000) and the K-SID (Kingston-Screening Inventory for Domestic Violence) (Gelles & Straus 1990 as cited in Campbell et al. 2005) were designed to screen offenders to predict their likelihood to re-assault.

<sup>34</sup> Researchers in criminology, public health and other fields have done predictive validation of many of these instruments including the Lethality Assessment (Campbell et al. 2005; Messing and Thaller 2013; Messing, Campbell, Wilson, et al. 2015). However, their focus is not on studying the effectiveness of the program in reducing or preventing future violence.

and unobservable ways from the agencies that do not adopt the program. Thus a cross-sectional approach could suffer from a selection bias.

In order to address this challenge and identify the causal effects of the Lethality Assessment Program on female homicide victimization, I exploit the within law enforcement agency variation in the timing of implementation of LAP in the state of Maryland. I use individual homicide level data from the FBI's Supplementary Homicide Reports (SHR) from 2000 to 2011 and aggregate them to the agency level in order to measure the effects of the program using a difference-in-differences framework. The implicit assumption made in this framework is that agencies that adopted the LAP early would have experienced similar rates of female homicide victimization as agencies that adopted the program late, in the absence of the LAP. I perform various robustness checks and falsification exercises to test the validity of this assumption. The fact that early and late adopting agencies track each other in the rates of female homicides prior to treatment and that the inclusion of neither the time-varying confounding factors nor agency-specific linear time trends changes the estimated effects of LAP support the validity of my research design.

Results indicate that the introduction of LAP significantly reduced female homicide victimization by males. Though the Supplementary Homicide Reports contain information about the relationship between the victim and the offender for a majority of the homicides, it is not clear which of the relationship categories might constitute an intimate partner



relationship.<sup>35</sup> Accordingly, research has shown that only about 71% of intimate partner homicides are identified in the SHR data (Langford, Isaac, and Kabat 1998). To avoid relying on a subjectively defined relationship status for identifying intimate partner homicides, I focus on an objectively defined category that is likely to contain a large number of intimate partner homicides. This is the group of homicides with a female victim, who was in the age group of 18 to 60 at the time of death, was killed by a male offender, and not during the commission of a robbery.<sup>36</sup> The details of the victim-offender relationships and circumstances of homicide in this category are shown in Figure C.1. According to Panel A over 55% of the victims in this category were either the wife or girlfriend of the offender. All of the circumstances listed in Panel B could be associated with an intimate partner homicide. The effect of LAP is also strong on this group of homicides. I find that LAP reduced female homicides committed by males by 35-45 percent which is equivalent to a reduction of 2-3 deaths for every 1 million population.

The central contribution of this paper is that I use a quasi-experimental difference-in-differences approach to identify the causal role of the most popular domestic violence victim screening protocol in reducing domestic violence related deaths. The results showing significant effects of the LAP in reducing female homicide victimization have

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<sup>35</sup> When the victim is defined as the wife or girlfriend of the offender it is clear that the relationship is an intimate one. However, in nearly 17% of the cases where a female is killed by a male, the relationship is defined as an acquaintance (Figure C.1, Panel A). In these cases the nature of the relationship between the victim and offender are not clear.

<sup>36</sup> To zero in on intimate partner homicides, from the group of homicides where a male killed a female within the age group of 18 to 60, I excluded robberies, burglaries, sniper attacks, and police killings of felons. I explicitly mention robberies in the text as they formed the largest category of circumstances that was excluded.

important policy implications. These results imply that a more proactive approach by police in screening of domestic violence victims and empowering the most high-risk victims with a safety plan and other resources, together with a close cooperation between law enforcement agencies and local domestic violence programs, can play a very important role in saving the victims' lives.

#### 4.2 Background on the Lethality Assessment Program

The Lethality Assessment Program – Maryland Model, was developed by the Maryland Network Against Domestic Violence in 2005. The main goal of this program is to provide law enforcement officers and other community professionals with a standardized evidence based tool to screen the victims of domestic violence and objectively identify those most at risk of being killed or seriously injured by their intimate partners (Lethality Assessment, MNADV).

The lethality assessment model consists of two parts: a lethality assessment questionnaire called the Lethality Screen for First Responders, which helps officers identify the victims with a high level of risk for lethality or near-lethality, and a referral protocol which is triggered when the victim is identified as high-danger.

The Lethality Screen (Figure C.3) consists of eleven yes or no questions which seek to assess the nature of the domestic abuse. An affirmative answer to at least one of the first three questions or four of the next eight questions would suggest an imminent danger to the safety and survival of the victim. This screening tool is based on Danger Assessment, a screening instrument developed by Dr. Jacquelyn Campbell, of The Johns Hopkins

University School of Nursing, to help counselors and clinicians in assessing a victim's risk of homicide or severe re-assault (Campbell 1994; Campbell et al. 2003).

When the officer identifies the victim as high-danger, he follows the referral protocol. As a first step, the officer conveys to the victim that she has been screened as high-risk and that other women in her situation have died (Messing et al. 2016). The officer then places a phone call to the local 24-hour domestic violence hotline in order to obtain a safety plan for the victim and encourages her to speak with the trained hotline advocate. Irrespective of the victim's decision to speak to the advocate, the officer informs the advocate about the victim's situation including her responses on the lethality screen (Messing, Campbell, Wilson, et al. 2015). The advocate then provides a safety plan that is tailored to the victims' circumstance, which is communicated to the victim whether or not she chooses to speak to the advocate. If the victim does speak with the advocate, in a brief conversation of approximately 10 minutes, the advocate attempts to gain the victim's trust, convince her of the imminent danger she faces, provide a safety plan and actively encourage her to seek help from the local shelter (Messing et al. 2016).

The implementation of LAP brings about many changes to the way domestic violence is addressed in the jurisdiction. Firstly, it provides police officers with an objective, well-defined protocol to handle domestic violence incidents. It also mandates the police officers to take a more pro-active approach in connecting the victim to the domestic violence services as opposed to a passive approach of providing information about the local shelters

and leaving the decision of getting help entirely to the victim.<sup>37</sup> Secondly, it fosters a strong relationship between law enforcement and the local domestic violence organization, thereby improving the preparedness of helplines and shelters to handle the volume of victims referred to them. Thirdly, at a victim level, implementation of LAP could significantly increase the victims' likelihood to reach out to the shelters and seek their services. Since domestic violence victims often tend to underestimate the risk they face (Messing and Thaller 2013), a tool such as the Lethality Screen might help victims better evaluate their risk of being killed or seriously injured and hence convince them to seek help.

#### 4.3 Data

The data on the timing of implementation of LAP by different law enforcement agencies is not available centrally with MNADV or any other associated organization. So, I collected this information for all the law enforcement agencies in Maryland serving a population of 10,000 or more.<sup>38</sup> The list of all such agencies along with their LAP start dates is presented in Table C.1. I was unable to get the LAP implementation date for four agencies so these are not included in the sample. All of the 39 agencies that are included in the sample implemented LAP between 2005 and 2012. I restrict the period of study

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<sup>37</sup> As per my email communication with Mr. Dave Sargent, Retired Police Lieutenant and Senior Program Manager at MNADV, there was no consistency in the police officers' approach to handling domestic violence calls prior to LAP. In most cases they would refer the victim to the nearest domestic violence program, and the referral could be written or verbal. This is also the typical response of law enforcement across the country.

<sup>38</sup> The sources of information include newsletters released by MNADV during the early days of LAP, individual law enforcement agencies, news articles, and the domestic violence helplines that are associated with specific agencies as part of the LAP.

from 2000 to 2011 during which 34 agencies started participating in LAP.

MNADV reported statistics on the number of lethality screens conducted and the take up rate among high-risk victims in Maryland between 2006 and 2009 (Maryland Network Against Domestic Violence 2010). I present these statistics in Table C.2. The LAP was active in 88 law enforcement agencies in Maryland by the end of 2009. Every year, around 220 victims reported domestic violence and were screened under LAP for every 100,000 population. Of them, slightly over half (120 for every 100,000 population) were identified as high-danger. About 70 women in every 100,000 population spoke to the domestic violence hotline advocate and about 20 of them sought services.

I use the Supplementary Homicide Reports of the FBI as the main source of homicide data. For every reported homicide, the SHR contains information on the date of the incident, reporting law enforcement agency, demographic information about the victims and offenders, and details about the relationship between them when available. Using this information, I construct an agency-level yearly panel on the counts of homicide within different categories.

The summary statistics are presented in Table C.3. Statistics on the counts of different types of homicide for every 100,000 population in an agency-year are reported in Panel A. On average, there are over 4 homicides per 100,000 in an agency-year. Majority of the homicide victims are male and only 22% are female. Nearly a third of all female victims were identified as the wife or girlfriend of the offender. About half of the female victims were between 18 to 60 years of age and were killed by a male offender under circumstances other than the commission of a robbery.

Finally, I have county level data on important confounding factors that might influence the rate of homicide such as demographic and economic characteristics, rate of policing per capita, and implementation of other policies to curtail intimate partner homicide. Descriptive statistics of these time-varying county-level control variables are in Panel B of Table C.3. The data on agency-level population estimates are from the FBI's Uniform Crime Reporting. County-level demographic variables have been calculated from the 1-year population estimates in the American Community Survey. Information on the poverty rate and median household income is from the Small Area Income and Poverty Estimates (U.S. Census Bureau), and the unemployment rate is from the Local Area Unemployment Statistics (Bureau of Labor Statistics). Agency level police counts obtained from the Uniform Crime Reports are aggregated to the county level to get the county level policing rates per 100,000 population. Information on the institution dates of Fatality Review Teams in different counties across Maryland are available in the Maryland Domestic Violence Fatality Review Council's newsletter (2012).

#### 4.4 Empirical Approach

To identify the effect of the Lethality Assessment Program, I exploit the within-agency variation in the implementation of the program across all law enforcement agencies in Maryland with a population of 10,000 or more. I implement a difference-in-differences model which tests whether female homicide rates change differently in jurisdictions that adopted LAP early than in jurisdictions that adopted it late. Formally I estimate the following equation:

$$Homicide_{at} = \beta_0 + \beta_1 LAP_{at} + \Omega X_{ct} + \theta_a + \tau_t + \varepsilon_{at} \quad (3.1)$$

where  $LAP_{at}$  is the proportion of year  $t$  during which agency  $a$  was participating in LAP,  $X_{ct}$  is the vector of county-level time-varying control variables and  $\theta_a$  and  $\tau_t$  are agency and year fixed effects respectively. The outcome variable  $Homicide_{at}$  is the count of homicides in the particular category in agency  $a$  in year  $t$ . Since this is a discrete variable and often there are zero female homicides in an agency-year cell, I estimate the model using a negative binomial regression by controlling for agency population on the right-hand side as the exposure variable. Robust standard errors are clustered at the agency level allowing for within-agency errors to be correlated over time.

In this difference-in-differences model, the underlying identifying assumption is that the rate of change of female homicides in jurisdictions that were late adopters of LAP provides a good counterfactual for the rate at which female homicides would have changed in the early adopting jurisdictions in the absence of LAP.

I test this identifying assumption and also relax it in several ways. First, I perform a statistical test to check if the rate of female homicide in early adopting (treated) agencies diverges from the trend seen in the late adopting (control) agencies even before the implementation of LAP. I do this by including an indicator for the year prior to adoption for the treated agencies while estimating equation 3.1. Failure to find a statistically significant difference in outcomes between the treated and control agencies prior to the adoption would lend support to the validity of the identifying assumption. I also extend this test to compare the divergence in the outcome variable between the treated and control agencies for multiple years before adoption. If the identifying assumption holds then we

would expect the difference between early and late adopters to be close to zero before the implementation of LAP.

Second, I test whether time-varying determinants of homicide rates are orthogonal to the within-agency variation in the adoption of LAP. If county-level observables, such as demographic composition, economic factors, and per-capita policing, vary differentially between the early and late adopting agencies alongside changes in the practice of LAP, then it would suggest that the female homicide rates might change in treated agencies differentially even in the absence of treatment. To ensure that this is not the case, I estimate equation 3.1 with and without these controls. I also include controls for the county-level incorporations of Fatality Review Teams to account for other contemporaneous policy changes within law enforcement agencies that might impact female homicides.<sup>39</sup> Under a properly specified model with a reasonable identifying assumption, the regressions without and with controls should yield similar estimates of the effects of LAP on female homicides.

Third, if along with implementing LAP, law enforcement agencies took other measures that affected the overall rate of homicide in early adopting agencies differently, then it would be a problematic to the identifying assumption. Hence, as a falsification exercise, I estimate the effect of implementing LAP on all homicides and homicides with

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<sup>39</sup> The Fatality Review Teams were instituted by different counties to review selected few domestic violence related deaths and identify possible changes to law enforcement practices that could prevent future occurrence of such incidents.



male victims. If the Lethality Assessment Program is not found to influence these outcomes, then it adds credence to the empirical approach.

Finally, I relax the identifying assumption by estimating the model with controls for agency-specific linear time trends. This would test if the results in the main model could be biased by any differential trends between the early and late adopting agencies.

## 4.5 Results

### *4.5.1 Effect of Lethality Assessment on Female Homicide Victimization*

First, I present graphical evidence corresponding to the main difference-in-differences model. By including indicators for up to two years prior to the implementation of LAP and allowing for dynamic treatment effects after in equation 3.1, I estimate the pre- and post- treatment divergence in outcomes between the treated and control agencies. These differences are plotted in Figure C.2. Panel A shows the divergence in the rate of homicides with female victims and male offenders. In panel B I present the divergence in the rate of homicides with female victims aged 18 to 60, male offenders, and the circumstance not identified as a robbery. In both panels the difference between early and late adopting agencies is close to zero prior to treatment (data points to the left of the solid vertical line). The fact that the control and treatment groups are tracking each other prior to treatment corroborates the validity of the identifying assumption. Whereas, in the post treatment period (data points to the right of the solid vertical line), there is a large drop in both outcomes in the agencies that adopted LAP early in comparison to those that didn't. This indicates that the introduction of LAP significantly reduced female homicide victimization.

I formally estimate the effect of LAP on female homicides using equation 3.1 and the results are presented in Table C.4. Panels A and B present results for the effect of LAP on all female homicides and on female homicides with male offenders respectively. Since I expect the effects of LAP to be concentrated on intimate partner homicides, in panel C, I present the effects of LAP on a narrowly defined category of female homicides. This category includes homicides where the victim was between 18 and 60 years of age, the offender was male, and the circumstance of homicide was not a robbery. Column 1 presents the estimates from the negative binomial regression with controls for agency and year fixed effects. I progressively control for time-varying county-level covariates in columns 2 through 5. Columns 6 and 7 present estimates from robustness checks, namely, testing for pre-treatment divergence by including the lead indicator for treatment and allowing for a relaxed identifying assumption by including agency-specific linear time trends.

In all the panels, the estimates change very little when I include controls or allow for agency-specific linear time trends. The coefficients on the lead indicator for treatment (LAP  $t-1$ ) are small and statistically indistinguishable from zero in all cases. These results are consistent with the assumption that the late-adopting agencies are good counterfactuals for those that adopted LAP early.

Results in panel A suggest that overall female homicide declined between 10 to 20 percent due to the implementation of LAP, but not all estimates are statistically different from zero. Since the intervention is particularly targeted towards homicides that result from domestic violence, this result is understandable. In panel B where the outcome

variable is the female homicides committed by men, I find a stronger effect. All estimates from the main model (columns 1 through 5) are statistically significant at the 1% level, and they indicate a 35 to 45 percent drop in female homicide victimization by men as a result of LAP. The effects are also pronounced in panel C where I only include homicides of women aged 18 to 60, by men, and under circumstances other than a robbery. All estimates from the main model are statistically significant at least at the 5% level, and indicate that LAP reduced such homicides by around 42 percent.

Research suggests that the rate of domestic violence could be different between married and cohabiting couples (e.g. Stets and Straus, 1989; Yllo and Straus, 1981). According to a report on intimate partner violence from the Bureau of Justice Statistics (BJS 1998) women in the age group of 16 to 24 experience the highest rate of violence. To the extent that younger women are less likely to be married this might suggest a higher rate of violence among unmarried couples. The rate of reporting abuse and utilizing domestic violence services could also be different between women that are married to their abusers and those that are not.<sup>40</sup> Because the effectiveness of LAP depends on both those factors, I test for heterogeneous effects of LAP on intimate partner homicide by the relationship between the victim and the offender. The results are in Table C.5. Panels A and B are restricted to female homicides where the victim was the girlfriend and wife of

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<sup>40</sup> According to Ms. Kelley Rainey, Director of Domestic Violence Services at the Family and Children's Services (FMC), Baltimore, MD, married women might have greater difficulty in leaving an abusive relationship because of several reasons (based on my phone communication with her). For instance, the finances may be more interlinked among married couples – the victim may not be able to get a bank account without getting noticed. Married women might also face a greater stigma while making the decision to leave their marriage because of domestic violence.

the male offender respectively.<sup>41</sup> As shown in panel A of Figure C.1, among the female victims aged 18 to 60 that were killed by men in a situation other than a robbery, nearly 17% were identified as acquaintances of the offender (third highest category after girlfriend and wife).<sup>42</sup> The nature of the relationship between the victim and offender is not clear in these cases. For instance, a couple that was dating or seeing each other occasionally at the time of the homicide might be classified as acquaintances. So I include this category as the third subgroup in panel C.<sup>43</sup> The estimates are large and statistically significant in panels A and C indicating that LAP is effective in saving lives of women who might have otherwise been killed by their boyfriends or male acquaintances. As for the women that are married to their abusers, estimates in panel B are positive and not statistically distinguishable from zero at the 5% level. This may be consistent with the idea that married women could be less willing or able to report domestic abuse or make attempts to escape an abusive relationship.

#### *4.5.2 Falsification: Effect of Lethality Assessment on all Homicides and Male Victims*

As seen in Table C.3, nearly 78% of all victims of homicide are male, and very few men are victims of intimate partner violence. If alongside LAP, law enforcement agencies take other steps to curb homicide or if homicide rates are declining differentially in early adopting agencies due to other external factors, then it would be difficult to disentangle

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<sup>41</sup> Homicides where the victim-offender relationship is defined as “Common-law Wife” and “Ex-wife” are included in the wife category.

<sup>42</sup> Homicides where the victim-offender relationship is defined as “Friend” and “Other known” are included in the acquaintance category.

<sup>43</sup> I limit this category to include only those cases where the victim was between the ages of 18 and 60 and there was no commission of a robbery.

the effects of LAP. To ensure that this is not the case, I repeat the same analysis for overall homicides and for homicides with male victims as a falsification exercise.

The results are presented in Table C.6. Panel A shows the results for overall homicides and Panel B for homicides with male victims. The estimated effects of LAP on these homicide categories are small and statistically indistinguishable from zero under most specifications. Estimates are positive (opposite in direction to the main results) and statistically significant under the basic specification with only the agency and year fixed effects in column 1. Since the introduction of LAP is not found to cause a significant decline in the types of homicide that ought not to be affected by LAP, it strengthens my confidence in the research design. These results indicate that no other contemporaneous factors that would affect homicides overall could explain the drop in the female homicide victimization by men that are seen in the previous sections.

#### *4.5.3 Test of Attrition: County-level Analysis*

A potential concern for this study is attrition. If a high-risk victim leaves her original agency jurisdiction as a result of the safety counselling she receives under LAP and is then killed by her intimate partner, I would be unable to identify such deaths and account for them under the appropriate agencies.<sup>44</sup> As a result, such an observed reduction of deaths in the jurisdiction due to migration after the introduction of LAP, would be falsely attributed to the program itself.

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<sup>44</sup> The Supplementary Homicide Reports do not contain information about the addresses of the offenders or victims. Even if such an address were to be recorded at the time of death, it might not be the original address where the victim was treated as a result of LAP.

In order to minimize the possible effects of attrition on the results, I repeat the empirical analysis at the county level. There are several reasons to expect the county-level analysis to effectively account for agency-level attrition, if any. Given that counties have a larger geographic area than the agency jurisdictions, it could be more likely that the mobility constrained victims of domestic violence remain within county lines after being treated under LAP, if not within agency lines. More importantly, agencies within a county are serviced by a domestic violence service program specifically assigned to that county. As per my communication with Ms. Kelley Rainey, Director of Domestic Violence Services at the Family and Children's Services (FMC), Baltimore, MD, victims are unwilling to leave their home in most cases. If they do move into a shelter, which is usually in an undisclosed location, it is almost always into the shelter operated by the domestic violence organization in their county of origin. This is because most of these shelters operate under full capacity, and a victim is transferred outside the county only under dire circumstances.

Results of the county-level analysis are presented in Table C.7. Because the implementation of LAP was at the agency level, treatment at the county level could be defined in multiple ways. In columns 1 and 2, the LAP treatment variable is defined as the proportion of the year for which the largest agency in the county had implemented LAP. In columns 3 and 4, the treatment variable is defined as the fraction of the county population treated in a given year, while adjusting for the month of implementation.<sup>45</sup> The

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<sup>45</sup> Information on the county-level breakdown of LAP implementation dates by agency and percentages of county population covered by the agencies is presented in Table C.8.

odd numbered columns represent specifications with only the year and county fixed effects, and the even numbered columns represent specifications with both fixed effects and controls. Panels A, B, and C contain results for all female homicides, female homicides by men, and the subgroup with victim in the age group of 18 to 60 and the circumstance not being a robbery respectively.

The county-level analysis yields results similar to those from the agency-level analysis. The rate of female homicide victimization by men is found to be 39 to 51 percent lower in counties after the implementation of LAP. If the high risk victims systematically moved out of the agency jurisdictions as a result of LAP but remained within the same county and got killed, then results at the county level would be more attenuated towards zero than results at the agency level. These results are consistent with the anecdotal evidence that very few victims are willing to leave their home to escape an abusive relationship.<sup>46</sup> Thus, agency-level attrition cannot explain the steep reduction in female homicides after the implementation of LAP.

#### 4.6 Conclusion

A shift in the public mindset about domestic violence from considering it a private family matter to identifying it as a crime and greater awareness about the extent of its damage led to an increase in various policies and programs aimed at addressing the problem in the early 1990s (Klein et al. 1997). Although the rates of intimate partner violence have declined since, it still remains the cause of over 35% of all female

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<sup>46</sup> In 2015, only 55 of the 461 high danger victims referred to FMC moved into a shelter.

homicides. Policies such as the mandatory and recommended arrest laws enacted to protect domestic violence victims by deterring or incapacitating the abuser have proved to be ineffective (Iyengar 2009). In contrast, this paper studies a police intervention called the Lethality Assessment Program that aims to reduce intimate partner homicide by identifying and empowering the most high-risk victims.

Using data on individual homicides from the Supplemental Homicide Reports, I exploit the within law enforcement agency variation in the implementation of the LAP in Maryland to identify the effects of the program in reducing domestic violence related fatalities. I find that this intervention reduced female homicide victimization by men by 35-45 percent. This reduction is equivalent to about 2-3 fewer deaths annually for every 1 million population. If this program were to be scaled nationally with the same success as in Maryland, the estimates suggest that as many as 600 lives could be saved every year across the country.



## 5. CONCLUSION

In this dissertation, I evaluate the effects of three public policies on important socio-economic outcomes. Since it is usually infeasible to conduct experiments to evaluate such public policies I employ three different quasi-experimental techniques to be able to claim causal inference.

I explore the causal role played by legal alcohol access in the spread of sexually transmitted diseases among the college-age population in the first chapter. Using administrative data on the number of gonorrhea cases diagnosed in men from California, I implement a regression discontinuity design to exploit the sudden change in legal alcohol access on an individual's 21<sup>st</sup> birthday. Intuitively, I compare the number of gonorrhea cases diagnosed in individuals just below and above age 21. Since we do not expect any other determinants of sexually transmitted diseases to change discontinuously on exactly the 21<sup>st</sup> birthday of the individual, we can attribute any increase in STDs at this age cut off to the increased drinking at the MLDA. Contrary to the findings of related literature, I find no evidence of an increase in STDs at the MLDA. This suggests that the general consensus that alcohol plays a causal role in the spread of STDs, at least among the college-age population, is likely to be driven by selection into drinking.

In the second chapter, we study the effect of the Section 8 housing voucher program on criminal outcomes of the adult recipients. By exploiting the variation in timing of enrollment that resulted from a randomized housing voucher lottery, we are able to identify the causal effects of the vouchers. We match two administrative datasets in this

project: arrest records from the Houston Police Department and data on the applicants to the Section 8 program along with their lottery and voucher use outcomes from the Houston Housing Authority. Since the lottery number influenced the timing of enrollment, we are able to compare the arrest outcomes of voucher recipients with low lottery numbers, who got the vouchers earlier, to those of recipients with high lottery numbers, who had to wait longer to get the voucher. Results indicate that the vouchers had unintended consequences. Individuals were more like to be arrested for violent crime after receiving the voucher, and this effect is mainly driven by people with a history of arrest and by men. Since the voucher do not move to neighborhoods that are much better than their pre-voucher neighborhoods, but receive a sizeable income transfer, we attribute these results to the income shock.

I assess the effectiveness of police intervention aimed at reducing intimate partner homicides, called the Lethality Assessment Program, in the third chapter. The LAP program, which was first developed in Maryland, has been adopted by law enforcement agencies across 34 states and has become the most popular domestic violence victim assistance program. As part of LAP, the victims are screened using a simple questionnaire and the most high-risk victims are identified. Following this, the law enforcement officer takes a proactive approach, calls the local domestic violence shelter, encourages the victim to seek help and provides her with a tailored safety plan. The program has multiple effects: it improves the response of law enforcement to domestic violence incidents, it fosters a close and cooperative relationship between law enforcement and the local domestic violence helpline, and it encourages the high-risk victims to seek help by providing them with an objective measure of the danger they face. In order to study the effects of this

program, I exploit the within-agency variation in the timing of implementation within Maryland in a difference-in-differences framework. Results indicate that the program reduced female homicide victimization by men by 35-45%.

Overall, I study three public policies and find that they could have a variety of both intended and unintended effects on society. I conclude that apart from considering the costs and benefits of such programs, it is important for policy makers to strive to understand the mechanisms through which the policies/programs might influence behavior and the heterogeneity in the behavioral responses to the policies from different types of individuals.

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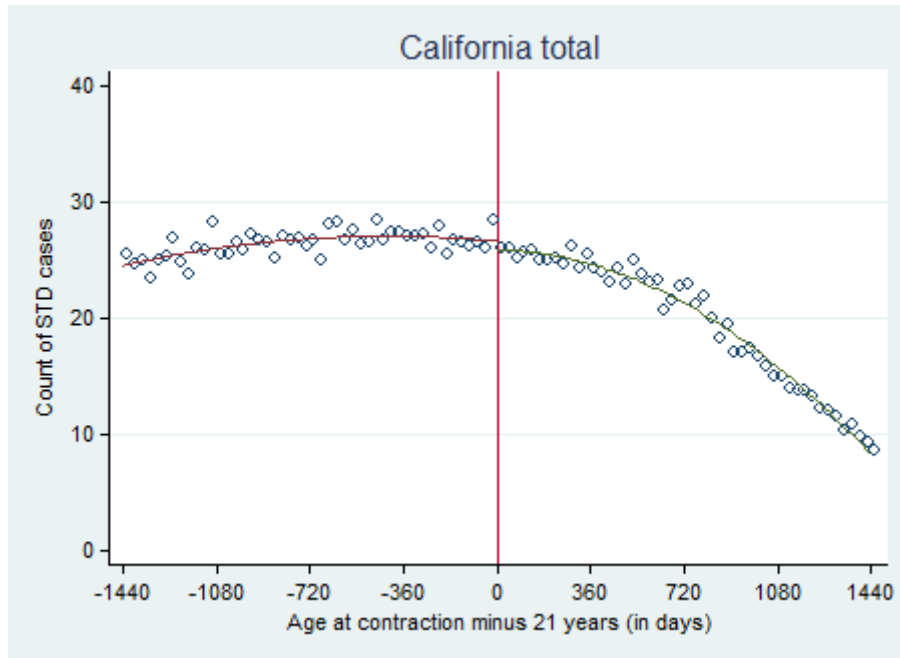
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## APPENDIX A

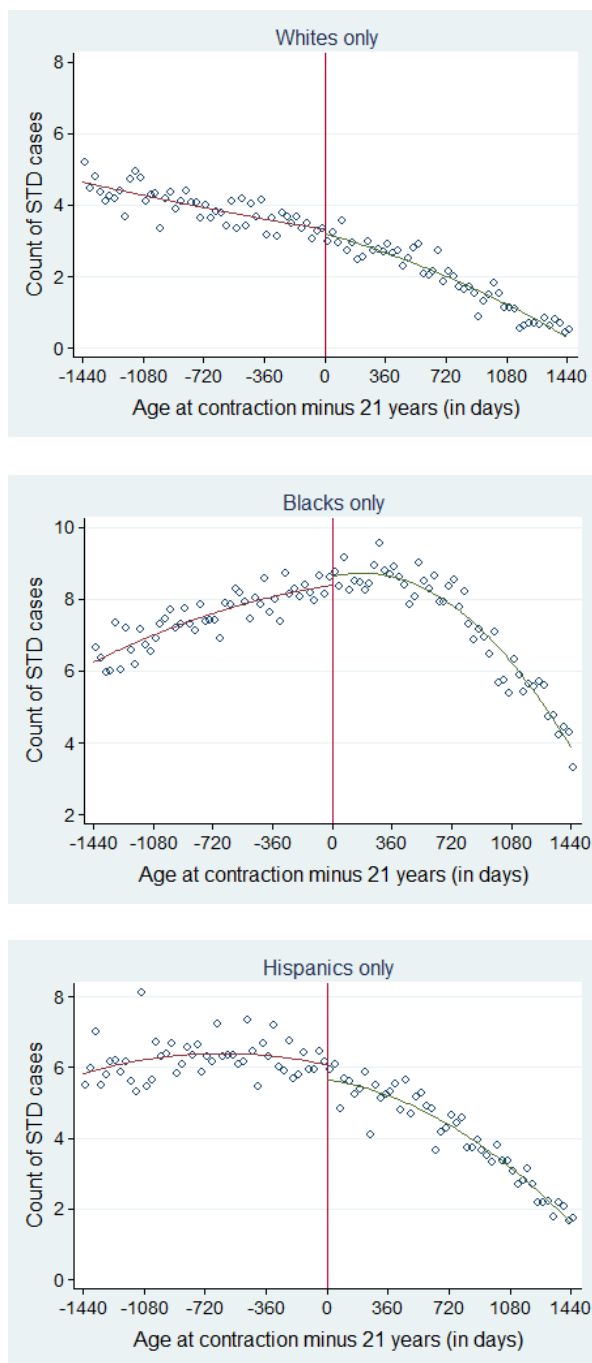
Figure A.1: Discontinuity in the count of STD cases at age 21



Notes: Each bubble represents the average count of STD cases in a 30 day block of age. The solid lines are second-order polynomials of age fitted to the count of cases on either side of the age 21 cutoff.

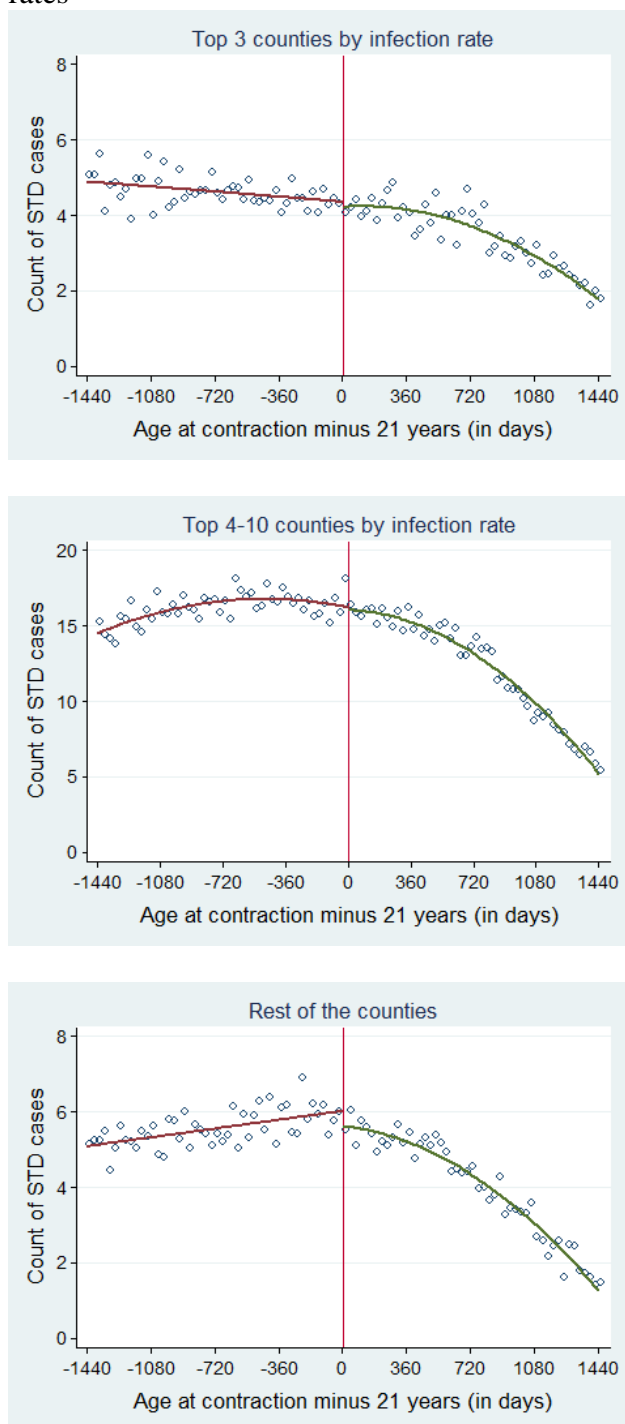


Figure A.2: Discontinuity in STDs at age 21 - By race



Notes: Each bubble represents the average count of STD cases for the particular race in a 30 day block of age. The solid lines are second-order polynomials of age fitted to the count of cases on either side of the age 21 cutoff.

Figure A.3: Discontinuity in STDs at age 21- By county groups with different infection rates



Notes: Each bubble represents the average count of STD cases for the particular group of counties in a 30 day block of age. The solid lines are second-order polynomials of age fitted to the count of cases on either side of the age 21 cutoff.

Table A.1: Descriptive statistics

Count of STD cases by exact age in days	Mean	Std. Dev	Range
Overall	23.07	(7.02)	3 - 44
Blacks	7.37	(2.94)	0 - 20
Whites	2.94	(2.10)	0 - 12
Hispanics	5.17	(2.64)	0 - 14
Other Races	7.59	(3.28)	0 - 20
Top 3 counties by infection rate	4.05	(2.16)	0 - 13
Top 4 to 10 counties by infection rate	14.23	(4.89)	1 - 30
Rest of the counties	4.78	(2.54)	0 - 18

Notes: All figures are derived from 67,443 cases of gonorrhea diagnosed in men of ages 17 to 25 between 2000 and 2012 in the state of California.

Table A.2: Infection rates in California

Panel A: 2012 infection rate statistics by race and age group									
	Total		Black		Hispanic		White		Other
	Cases	Rate	Cases	Rate	Cases	Rate	Cases	Rate	Cases
California total	33,778	89.3	7,257	329.3	8,447	58.2	7,691	51.4	10,383
Males total	20,544	109.2	3,939	361.2	5,114	69.8	5,418	72.7	6,073
Males by age group:									
Ages 0 to 9	6	0.2	0	0.0	3	0.2	0	0.0	3
Ages 10 to 14	35	2.7	13	17.7	12	1.8	0	0.0	10
Ages 15 to 19	1,830	127.6	611	682.1	500	71.8	187	43.6	532
<b>Ages 20 to 24</b>	<b>5,075</b>	<b>344.7</b>	<b>1,200</b>	<b>1283.0</b>	<b>1,407</b>	<b>208.6</b>	<b>895</b>	<b>187.6</b>	<b>1,573</b>
Ages 25 to 29	4,388	309.7	808	1014.2	1,242	198.9	1,112	224.5	1,226
Ages 30 to 34	3,044	223.2	451	594.0	807	137.1	914	188.9	872
Ages 35 to 44	3,405	132.1	459	312.3	798	75.3	1,138	121.0	1,010
Ages 45+ or unknown	2,761	39.5	397	98.4	345	20.1	1,172	32.4	748

Panel B: Infection rates over time among males of ages 15 to 24 in the top 10 counties (by infection rate)										
	2008		2009		2010		2011		2012	
	Cases	Rate	Cases	Rate	Cases	Rate	Cases	Rate	Cases	Rate
California total	5,569	187.3	5,327	175.8	5,906	203.5	5,912	203.4	6,905	237.6
<b>San Francisco</b>	<b>279</b>	<b>935.9</b>	<b>227</b>	<b>777.1</b>	<b>249</b>	<b>526.8</b>	<b>319</b>	<b>695.7</b>	<b>355</b>	<b>790.6</b>
Sacramento	401	362.0	403	358.7	473	449.2	396	376.4	413	393.7
Alameda	465	462.0	385	378.0	445	418.5	352	332.9	314	297.5
Kern	225	284.4	189	236.7	242	328.4	211	286.4	328	444.9
Fresno	118	138.1	163	188.9	184	228.6	243	302.5	292	363.4
<b>Los Angeles</b>	<b>2,052</b>	<b>259.9</b>	<b>2,133</b>	<b>261.7</b>	<b>2,222</b>	<b>287.7</b>	<b>2,248</b>	<b>293.5</b>	<b>2,615</b>	<b>344.5</b>
San Joaquin	184	294.4	136	213.9	158	282	131	229	149	255.4
San Bernardino	308	162.7	272	140.9	275	156.6	336	189.7	430	241.9
Contra Costa	133	167.9	124	154.8	157	225.8	149	208.6	159	216.7
San Diego	433	177.3	403	161.8	474	177.5	486	183.6	488	186.3
All other counties	971		892		1,027		1,041		1,362	

Notes: Rates are per 100,000 population. All figures are from the data table for gonorrhea published by the California Department of Public Health, STD Control Branch (data reported through 08/19/2013).

Table A.3: Effect of the MLDA on STDs - Discontinuity at age 21

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Panel A: Log linear</b>	log(STD count by exact age in days)							
Older than 21	-0.0423 (0.0300)	-0.0449 (0.0316)	-0.00394 (0.0337)	-0.00393 (0.0362)	-0.0191 (0.0307)	-0.0200 (0.0326)	-0.0280 (0.0291)	-0.0452 (0.0386)
<b>Panel B: Negative Binomial</b>	STD count by exact age in days							
Older than 21	-0.0414 (0.0290)	-0.0452 (0.0305)	-0.0122 (0.0327)	-0.0141 (0.0350)	-0.0228 (0.0301)	-0.0250 (0.0319)	-0.0312 (0.0283)	-0.0425 (0.0377)
Observations	2915	2901	2191	2177	1461	1447	731	365
Bandwidth (months)	+/- 48	+/- 48	+/- 36	+/- 36	+/- 24	+/- 24	+/- 12	+/- 6
Age polynomial	Cubic	Cubic	Cubic	Cubic	Quadratic	Quadratic	Linear	Linear
Cases diagnosed in the 2 weeks after 21 <sup>st</sup> birthday	Yes	No	Yes	No	Yes	No	Yes	Yes

Notes: Each cell represents results from a separate regression estimating equation 1.2. Unit of observation is the normalized age in days. Panel A presents results from log-linear regressions where the dependent variable is the log of the STD count. Panel B presents results from negative binomial regressions where the dependent variable is the STD count. Robust standard errors are presented in parenthesis.

Table A.4: Discontinuity in STDs at age 21 - By race

	(1)	(2)	(3)	(4)	(5)
<b>Panel A: Whites</b>	STD Count among Whites				
Older than 21	-0.0860 (0.0823)	-0.0432 (0.0927)	-0.0477 (0.0848)	-0.0418 (0.0773)	0.0365 (0.110)
<b>Panel B: Blacks</b>	STD Count among Blacks				
Older than 21	-0.00309 (0.0513)	0.0197 (0.0585)	0.0230 (0.0534)	-0.00763 (0.0503)	0.0201 (0.0704)
<b>Panel C: Hispanics</b>	STD Count among Hispanics				
Older than 21	-0.0522 (0.0604)	-0.0564 (0.0677)	-0.0713 (0.0633)	-0.0401 (0.0596)	-0.0510 (0.0782)
Observations	2915	2191	1461	731	365
Bandwidth (months)	+/- 48	+/- 36	+/- 24	+/- 12	+/- 6
Age polynomial	Cubic	Cubic	Quadratic	Linear	Linear

Notes: Each cell represents results from a separate regression estimating equation 1.2. Unit of observation is the normalized age in days. Panel A, B and C present results for Whites, Blacks and Hispanics respectively. All results are from negative binomial regressions where the dependent variables are the STD counts in each of the race categories. Robust standard errors are presented in parenthesis.

Table A.5: Discontinuity in STDs at age 21 - By county groups with different infection rates

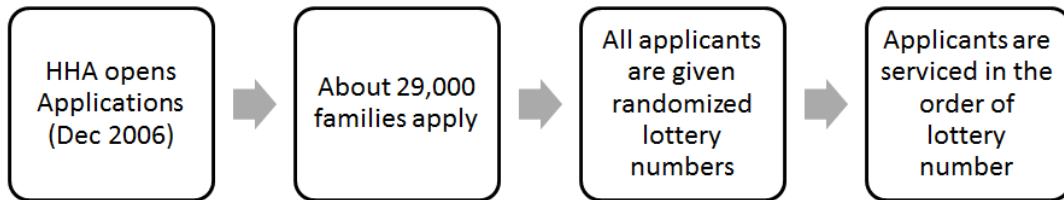
	(1)	(2)	(3)	(4)	(5)
<b>Panel A: Top 3 counties</b>	STD count by exact age in days				
Older than 21	-0.00956 (0.0708)	-0.0199 (0.0803)	0.00854 (0.0733)	-0.0488 (0.0705)	-0.0112 (0.0967)
<b>Panel B: Top 4-10 counties</b>	STD count by exact age in days				
Older than 21	-0.0401 (0.0388)	-0.000712 (0.0440)	-0.0213 (0.0405)	-0.0262 (0.0379)	-0.0654 (0.0521)
<b>Panel C: Remaining counties</b>	STD count by exact age in days				
Older than 21	-0.0682 (0.0673)	-0.0388 (0.0779)	-0.0514 (0.0715)	-0.0313 (0.0656)	-0.00133 (0.0962)
Observations	2915	2191	1461	731	365
Bandwidth (months)	+/- 48	+/- 36	+/- 24	+/- 12	+/- 6
Age polynomial	Cubic	Cubic	Quadratic	Linear	Linear

Notes: Each cell represents results from a separate regression estimating equation 1.2. Unit of observation is the normalized age in days. The panels present results for groups of counties from the highest to lowest infection rates. Panel A is for the top 3 counties with the highest infection rates in California. Panel B is for the top 4 to 10 counties by infection rate and Panel C is for the rest of the counties. All results are from negative binomial regressions where the dependent variables are the STD counts in each group of counties. Robust standard errors are presented in parenthesis.

## APPENDIX B

Figure B.1: Lottery and voucher service processes

a. Lottery process



b. Voucher service process

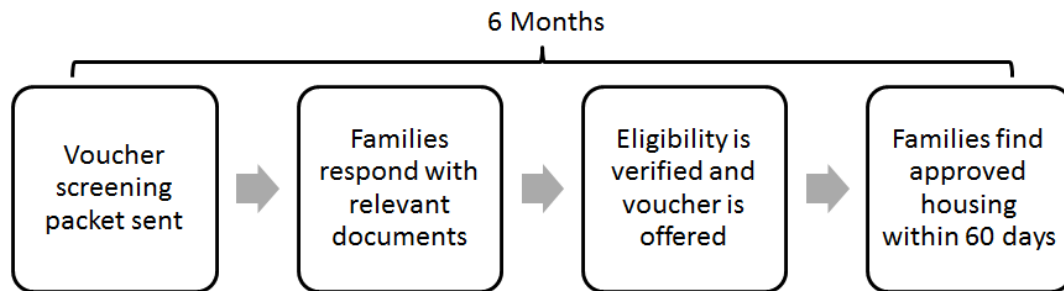
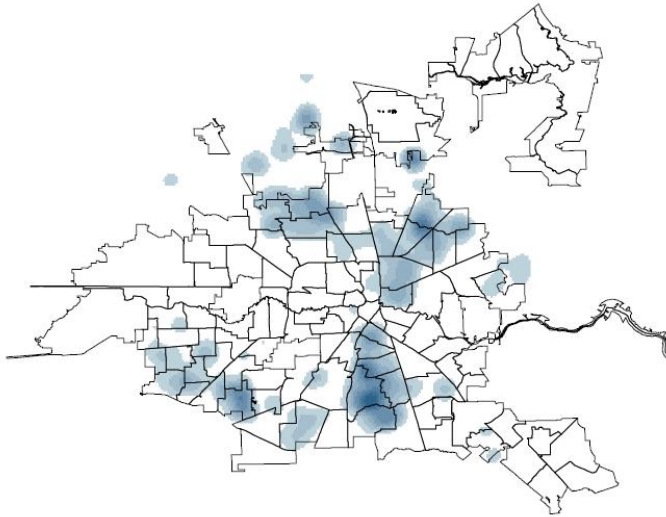


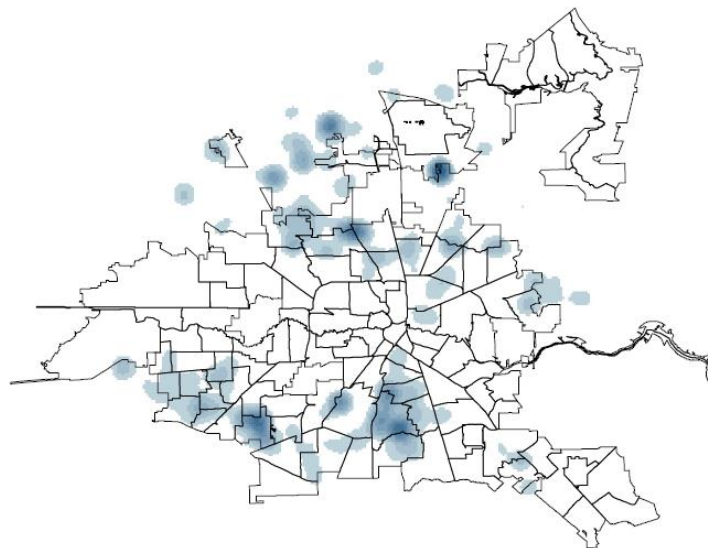


Figure B.2: Heat maps of application and voucher use addresses

a. Distribution of Application Addresses

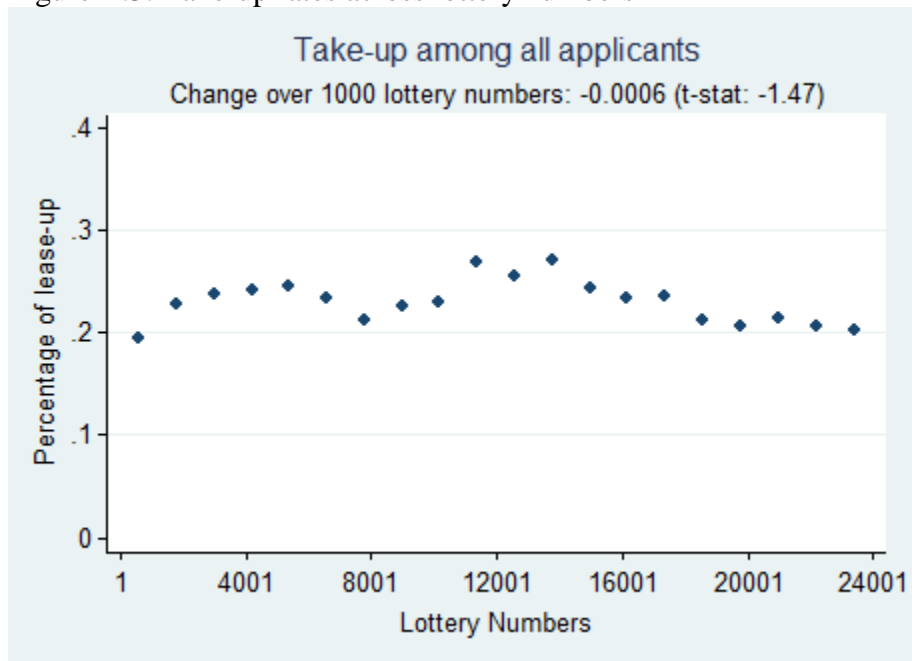


b. Distribution of Voucher Use Addresses



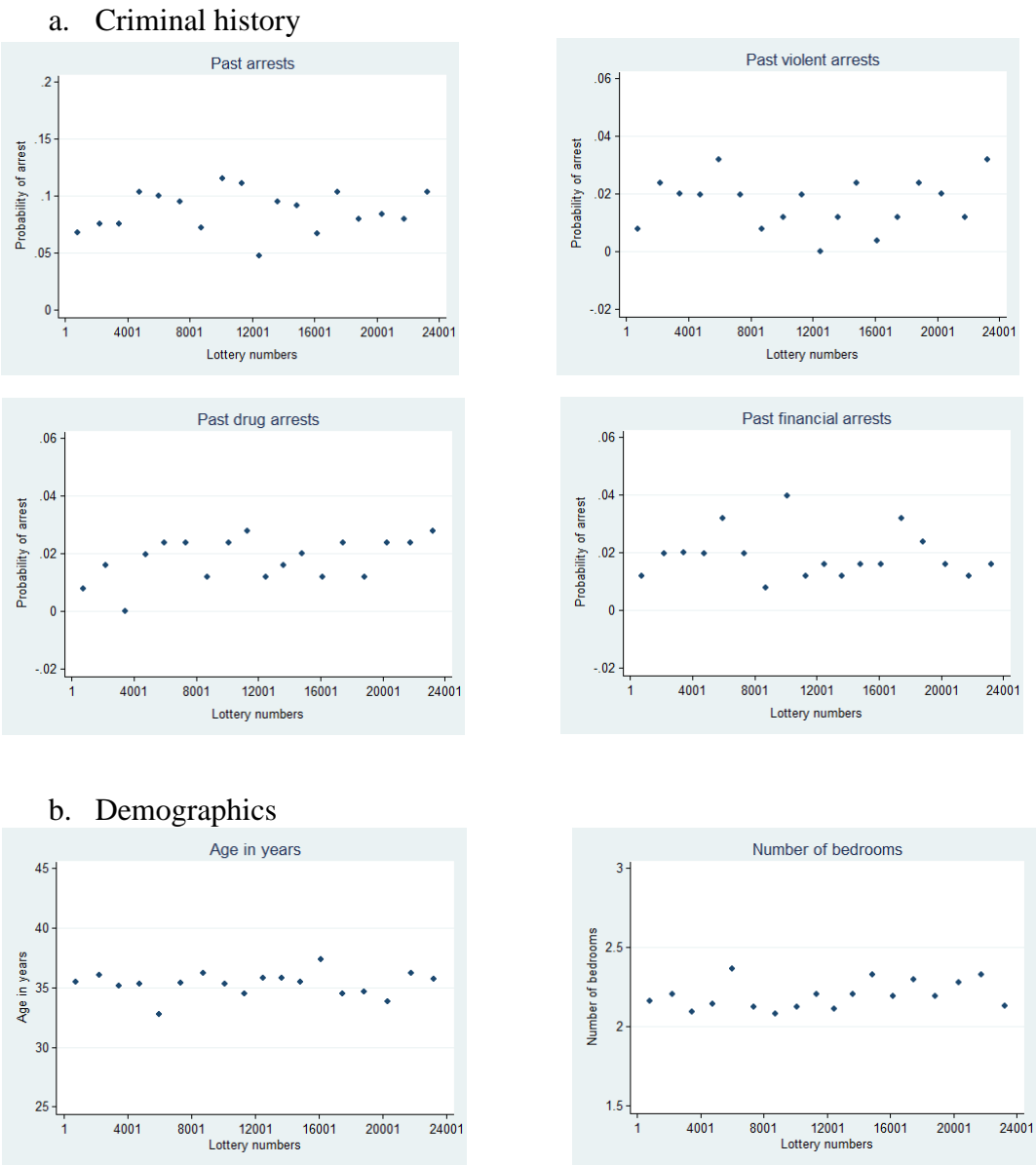
Notes: The heat maps are created in ArcMap using a point density operation that creates a grid over the map and then counts the number of address points within each grid cell. The outline indicates the boundaries of the police beats of the Houston Police Department.

Figure B.3: Take-up rates across lottery numbers



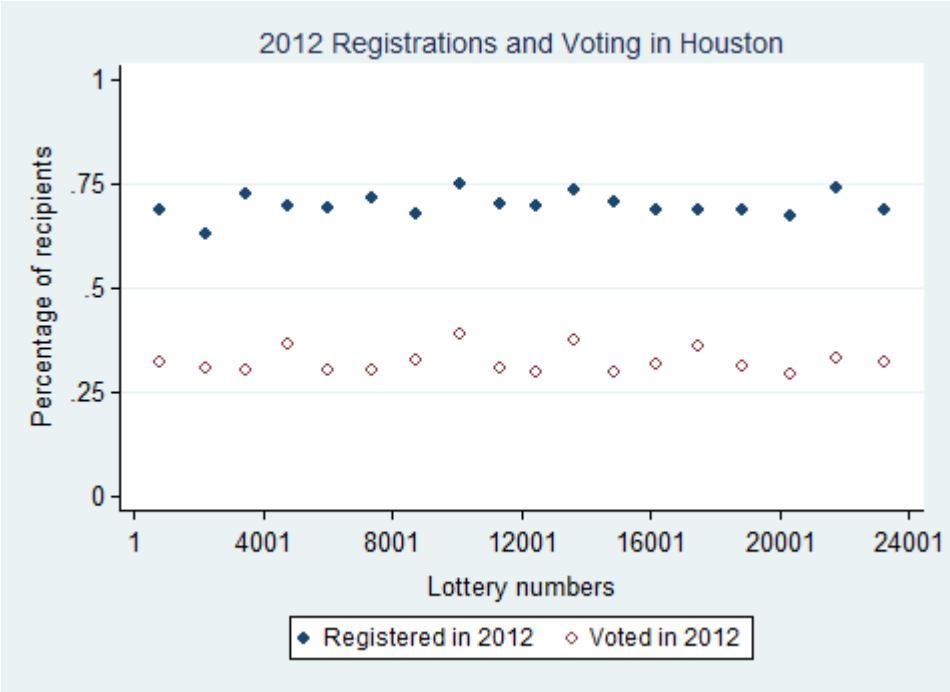
Notes: Each bubble represents the percentage of lease-up within bins of about 980 applicants.

Figure B.4: Test of randomization: Distribution of pre-lottery characteristics



Notes: Each bubble represents the local average of the variable within bins of about 250 individuals. Criminal history variables represent the probability of arrest in the crime category between 2002 and 2006.

Figure B.5: Test for attrition - Likelihood of voter registration and voting in Houston in 2012 across lottery numbers



Notes: Each bubble represents the local percentage of recipients that were registered to vote and that voted in Houston in 2012 within bins of about 250 individuals.

Table B.1: Comparison of application and voucher use addresses for recipients

Panel A: Voucher Use Characteristics		Mean (s.d.)	
Distance moved in miles		4.7 (5.5)	
Rent paid by voucher		628 (253)	
Rent paid by resident		205 (203)	
Percent living in public housing before		3.4 (0.2)	
Observations		1693	

Panel B: Neighborhood Characteristics	Application Address	Voucher Use Address	Difference
Census Tract Characteristics			
Median age	31.7 (4.8)	30.7 (4.5)	-1.0*** (0.2)
Percent over 18 years	70.7 (5.0)	69.7 (4.8)	-1.0*** (0.2)
Percent male	48.0 (3.1)	47.9 (3.0)	-0.1 (0.1)
Percent white	26.5 (18.0)	30.1 (17.9)	3.6*** (0.6)
Percent black	52.5 (27.1)	47.1 (26.4)	-5.4*** (0.9)
Percent Hispanic	35.4 (21.4)	37.9 (21.0)	2.5*** (0.7)
Median rent	797 (168)	836 (181)	39*** (6)
Percent housing occupied	86.9 (7.3)	87.7 (7.0)	0.8*** (0.2)
Percent unemployment	12.3 (5.6)	11.1 (5.4)	-1.2*** (0.2)
Median household income	33213 (12329)	35727 (13505)	2514*** (444)
Median family income	37637 (14950)	39446 (14791)	1809*** (511)
Percent below poverty	34.6 (15.9)	32 (16.0)	-2.6*** (0.5)
Observations	1693	1693	
Police Division Characteristics (Annual rates per 1000 population)			
Crime rate	135.9 (23.3)	133.8 (25)	-2.1** (0.8)
Murder rate	0.2 (0.0)	0.2 (0.0)	0.0 (0.0)
Violent crime rate	13.5 (3.0)	13.2 (3.4)	-0.3*** (0.1)
Property crime rate	58.9 (10.8)	58.5 (11.0)	-0.4 (0.4)
Observations	1389	1176	

Notes: Statistics are shown for voucher recipients for whom both pre and post-lottery addresses were available and geocodable. Crime rates at the police division level are from 2000 to 2005.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

Table B.2: Pre-lottery descriptive statistics

	All			Low Lottery Numbers	High Lottery Numbers	Difference
	Observations	Mean (s.d.)	Range	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)
<b>Lottery Variables</b>						
Lottery number	4510	11852 (6734)	8 - 23980	6078 (3422)	17625 (3507)	-11547*** (103)
Voucher service quarter	4510	12.9 (3.3)	8 - 17	10.0 (2.2)	15.8 (0.7)	-5.8*** (0.0)
<b>HHH Characteristics</b>						
Age (in years)	4510	35.3 (14.2)	16 - 97	35.1 (14.2)	35.5 (14.1)	-0.4 (0.4)
Number of bedrooms	4510	2.20 (0.96)	1 - 8	2.17 (0.93)	2.23 (0.98)	-0.06** (0.03)
Male	3844	0.12 (0.29)	0 - 1	0.12 (0.30)	0.11 (0.28)	0.01 (0.01)
Black	2612	0.94 (0.24)	0 - 1	0.94 (0.24)	0.94 (0.23)	0.00 (0.01)
White	2612	0.03 (0.18)	0 - 1	0.03 (0.18)	0.03 (0.18)	0.00 (0.01)
Homeless at the time of admission	2612	0.00 (0.03)	0 - 1	0.00 (0.04)	0.00 (0.03)	0.00 (0.00)
Arrested in 5 years prior to lottery	4510	0.09 (0.28)	0 - 1	0.09 (0.29)	0.08 (0.28)	0.01 (0.01)
Violent offense in 5 years prior	4510	0.02 (0.13)	0 - 1	0.02 (0.13)	0.02 (0.12)	0.00 (0.00)
Drug offense in 5 years prior	4510	0.02 (0.13)	0 - 1	0.02 (0.13)	0.02 (0.14)	0.00 (0.00)
Financial offense in 5 years prior	4510	0.02 (0.14)	0 - 1	0.02 (0.14)	0.02 (0.13)	0.00 (0.00)
Arrested between 1990 and 2006	4510	0.20 (0.40)	0 - 1	0.20 (0.40)	0.19 (0.39)	0.01 (0.01)
<b>Neighborhood Characteristics</b>						
Percent black in Census Tract	3633	51.4 (27.1)	0.7 - 94.8	51.1 (26.5)	51.8 (27.7)	-0.7 (0.9)
Unemployment rate in Census Tract	3633	12.1 (5.5)	0 - 32.4	11.8 (5.4)	12.3 (5.6)	-0.4** (0.2)
Median household income in Census Tract	3633	33775 (12806)	9926 - 154375	33489 (12381)	34058 (13212)	-570 (425)
Poverty rate in Census Tract	3633	34.3 (15.9)	0 - 81.9	34.8 (15.7)	33.7 (16.1)	1.1** (0.5)
Crime rate per 1k population	2938	135.1 (23.8)	76.1 - 165.5	134.3 (24.7)	135.8 (22.9)	-1.4 (0.9)
Violent crime rate per 1k population	2938	13.4 (3.1)	6.7 - 16.9	13.3 (3.3)	13.5 (3.0)	-0.2* (0.1)
Property crime rate per 1k population	2938	58.6 (10.7)	39.3 - 77.4	58.4 (10.8)	58.7 (10.7)	-0.4 (0.4)

Notes: Lottery numbers are classified as low or high based on whether they are below or above the median (11896). Neighborhood crime rates are annual rates reported at the police division level from 2000 to 2005.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

Table B.3: Post-lottery descriptive statistics [2010 Q1 to 2011 Q3]

	All		Low Lottery Numbers	High Lottery Numbers	Difference
	Mean (s.d.)	Range	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)
Post voucher service	0.532 (0.499)	0 - 1	0.889 (0.314)	0.174 (0.379)	0.715*** (0.004)
Post lease-up with voucher	0.517 (0.500)	0 - 1	0.866 (0.341)	0.168 (0.374)	0.698*** (0.004)
Probability of arrest in a quarter	0.006 (0.079)	0 - 1	0.007 (0.084)	0.005 (0.074)	0.002* (0.001)
Probability of violent arrest in a quarter	0.001 (0.028)	0 - 1	0.001 (0.033)	0.000 (0.021)	0.001** (0.000)
Probability of drug arrest in a quarter	0.001 (0.033)	0 - 1	0.001 (0.036)	0.001 (0.030)	0.000 (0.000)
Probability of financial arrest in a quarter	0.001 (0.034)	0 - 1	0.001 (0.037)	0.001 (0.031)	0.000 (0.000)
Observations	31570		15785	15785	
Individuals	4510		2255	2255	

Notes: Lottery numbers are classified as low or high based on whether they are below or above the median (11896). Unit of observation is a person-quarter. Statistics are derived from all the quarters after 2009.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

Table B.4: Test of randomization

Dependent variables	Observations	(1)	(2)
		Lottery number/1000	Voucher service quarter
Arrested in 5 years prior to lottery	4510	0.000280 (0.000617)	0.000327 (0.00127)
Violent offense in 5 years prior	4510	0.0000408 (0.000305)	-0.000164 (0.000602)
Drug offense in 5 years prior	4510	0.000461 (0.000294)	0.000907 (0.000596)
Financial offense in 5 years prior	4510	-0.0000880 (0.000292)	-0.000367 (0.000618)
Number of arrests in 5 years prior	4510	0.000828 (0.000897)	0.00164 (0.00180)
Number of violent arrests in 5 years prior	4510	0.000164 (0.000322)	0.000111 (0.000640)
Number of drug arrests in 5 years prior	4510	0.000527 (0.000373)	0.00112 (0.000755)
Number of financial arrests in 5 years prior	4510	0.000127 (0.000337)	0.000167 (0.000721)
Arrested between 1990 and 2006	4510	0.000334 (0.000877)	0.000505 (0.00179)
Age	4510	0.0109 (0.0312)	0.0405 (0.0638)
Number of bedrooms	4510	0.00455** (0.00211)	0.00880** (0.00428)
Male	3844	-0.000362 (0.000701)	-0.00106 (0.00143)
Black	2612	0.000439 (0.000711)	0.000930 (0.00147)
Percent black in Census Tract	3633	0.0720 (0.0661)	0.241* (0.135)
Unemployment rate in Census Tract	3633	0.0287** (0.0136)	0.0758*** (0.0278)
Median household income in Census Tract	3633	24.34 (31.22)	58.21 (63.59)
Poverty rate in Census Tract	3632	-0.0686* (0.0392)	-0.105 (0.0801)
Crimes per 1k population	2938	0.148** (0.0652)	0.406*** (0.136)
Violent crimes per 1k population	2938	0.0194** (0.00861)	0.0537*** (0.0179)
Property crimes per 1k population	2938	0.0428 (0.0291)	0.109* (0.0604)

Notes: Each cell represents a separate regression, estimating equation 2.1 with the observed covariates as the dependent variables. Unit of observation is an individual. Column 1 shows the coefficients of lottery number scaled down by 1000 and column 2 shows coefficients of the quarter in which the voucher is serviced. Robust standard errors are presented in parentheses.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level



Table B.5: First stage - Relationship between voucher service and lease-up

	(1)	(2)
	Post Lease-up with Voucher	
Post voucher service	0.849*** (0.00394)	0.849*** (0.00395)
Observations	85690	85690
Individuals	4510	4510
Quarter FE	Yes	Yes
Controls	No	Yes

Notes: Each column represents a separate regression estimating equation 2.2 with the indicator for post lease-up as the dependent variable. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the 5 years prior to the lottery. Unit of observation is a person-quarter. Robust standard errors, clustered at the individual level, are presented in parentheses.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

Table B.6: Effect of voucher service on crime - By crime type

	OLS		Negative Binomial	
	(1)	(2)	(3)	(4)
<b>Panel A: All Arrests</b>				
Post voucher service	0.000487 (0.000975)	0.000460 (0.000964)	0.0758 (0.151)	0.0689 (0.150)
Pre-lottery mean	0.0055			
<b>Panel B: Violent Arrests</b>				
Post voucher service	0.000685** (0.000349)	0.000676* (0.000347)	0.787** (0.376)	0.776** (0.376)
Pre-lottery mean	0.0007			
<b>Panel C: Drug Arrests</b>				
Post voucher service	0.0000780 (0.000384)	0.000133 (0.000379)	0.0766 (0.374)	0.131 (0.369)
Pre-lottery mean	0.0012			
<b>Panel D: Financial Arrests</b>				
Post voucher service	0.000191 (0.000427)	0.000157 (0.000424)	0.149 (0.330)	0.115 (0.329)
Pre-lottery mean	0.0007			
Observations	85690	85690	85690	85690
Individuals	4510	4510	4510	4510
Quarter FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Notes: Each column within a panel represents a separate regression estimating equation 2.2. The dependent variables in Panels A through D are dummy variables indicating an arrest in the person-quarter for any offense, violent offense, drug related offense, and financially motivated offense respectively. Pre-lottery mean is the mean of quarterly probability of arrest in the crime category from the year 2006. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the crime category in the 5 years prior to the lottery. Robust standard errors, clustered at the individual level, are presented in parentheses.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

Table B.7: Effect of voucher service on crime - By time since voucher service

	(1)	(2)
<b>Panel A: All Arrests</b>		
< 1 yr since voucher service	0.00108 (0.00105)	0.00106 (0.00104)
> 1 yr since voucher service	-0.000607 (0.00128)	-0.000645 (0.00127)
Pre-lottery mean	0.0055	
<b>Panel B: Violent Arrests</b>		
< 1 yr since voucher service	0.000739** (0.000361)	0.000733** (0.000360)
> 1 yr since voucher service	0.000586 (0.000474)	0.000570 (0.000471)
Pre-lottery mean	0.0007	
<b>Panel C: Drug Arrests</b>		
< 1 yr since voucher service	0.000245 (0.000416)	0.000290 (0.000413)
> 1 yr since voucher service	-0.000230 (0.000516)	-0.000160 (0.000509)
Pre-lottery mean	0.0012	
<b>Panel D: Financial Arrests</b>		
< 1 yr since voucher service	0.000303 (0.000499)	0.000276 (0.000497)
> 1 yr since voucher service	-0.0000175 (0.000453)	-0.0000619 (0.000450)
Pre-lottery mean	0.0007	
Observations	85690	85690
Individuals	4510	4510
Quarter FE	Yes	Yes
Controls	No	Yes

Notes: Each column within a panel represents a separate regression estimating a version of equation 2.2 with the independent variable split up by duration since voucher service. Pre-lottery mean is the mean of quarterly probability of arrest in the crime category from the year 2006. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the crime category in the 5 years prior to the lottery. Robust standard errors, clustered at the individual level, are presented in parentheses.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

Table B.8: Effect of voucher service on crime - Subgroup analysis

	Criminal history		Gender	
	Past arrest (1)	No past arrest (2)	Males (3)	Females (4)
<b>Panel A: All arrests</b>				
Post Voucher Service	0.00180 (0.00349)	0.000162 (0.000840)	-0.00137 (0.00437)	-0.000302 (0.000976)
Pre-lottery mean	0.0281	0	0.0174	0.0039
<b>Panel B: Violent arrests</b>				
Post Voucher Service	0.00254** (0.00125)	0.000230 (0.000307)	0.00378* (0.00214)	-0.0000427 (0.000309)
Pre-lottery mean	0.0037	0	0.0013	0.0005
<b>Panel C: Drug arrests</b>				
Post Voucher Service	-0.0000596 (0.00156)	0.000184 (0.000278)	-0.00148 (0.00203)	0.0000476 (0.000379)
Pre-lottery mean	0.0062	0	0.006	0.0008
<b>Panel D: Financial arrests</b>				
Post Voucher Service	0.00155 (0.00160)	-0.000175 (0.000353)	-0.00146 (0.00149)	0.000423 (0.000451)
Pre-lottery mean	0.0037	0	0.0007	0.0006
Observations	16739	68951	7106	61693
Individuals	881	3629	374	3247
Quarter FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Each column within a panel represents a separate regression estimating equation 2.2 within a subgroup. Every pair of columns present heterogeneous effects of the vouchers within subgroups by criminal history, gender, and change of address respectively. The dependent variables in Panels A through D are dummy variables indicating an arrest in the person-quarter for any offense, violent offense, drug related offense, and financially motivated offense respectively. Pre-lottery mean is the mean of quarterly probability of arrest in the crime category for the particular subgroup from the year 2006. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the crime category in the 5 years prior to the lottery. Robust standard errors, clustered at the individual level, are presented in parentheses.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

Table B.9: Test of differential attrition across lottery numbers - Registration and voting in 2012

	(1)	(2)
<b>Panel A</b>	Registered	Voted
Lottery number/1000	0.000520 (0.00102)	-0.0000686 (0.00103)
<b>Panel B</b>	Registered	Voted
Quarter of voucher service	0.000521 (0.00208)	-0.000601 (0.00211)
Observations	4510	4510

Notes: Each cell represents a separate regression, estimating equation 2.1 with indicators for being registered and having voted in 2012 as the dependent variables in columns 1 and 2, respectively. Unit of observation is an individual. Panel A shows the coefficients for lottery number scaled down by 1000 and Panel B shows coefficients for the voucher service quarter. Robust standard errors are presented in parentheses.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

Table B.10: Intent to treat estimates with controls and leads

	(1)	(2)	(3)
<b>Panel A: All Arrests</b>			
Post voucher service	0.000487 (0.000975)	0.000460 (0.000964)	0.000597 (0.00110)
Announcement effect			0.000269 (0.00121)
Lead			0.000214 (0.00106)
<b>Panel B: Violent Arrests</b>			
Post voucher service	0.000685** (0.000349)	0.000676* (0.000347)	0.000870** (0.000385)
Announcement effect			0.000727* (0.000427)
Lead			-0.000137 (0.000362)
<b>Panel C: Drug Arrests</b>			
Post voucher service	0.0000780 (0.000384)	0.000133 (0.000379)	0.000504 (0.000438)
Announcement effect			0.000888 (0.000552)
Lead			0.000380 (0.000471)
<b>Panel D: Financial Arrests</b>			
Post voucher service	0.000191 (0.000427)	0.000157 (0.000424)	0.000454 (0.000458)
Announcement effect			0.000484 (0.000473)
Lead			0.000593 (0.000495)
Observations	85690	85690	85690
Individuals	4510	4510	4510
Quarter FE	Yes	Yes	Yes
Controls	No	Yes	Yes

Notes: Each column within a panel represents a separate regression estimating equation 2.2. In column 3, indicators for 1-2 quarters before voucher service (announcement effect) and 3-4 quarters before voucher service (leads testing for pre-treatment trends) are included. Robust standard errors, clustered at the individual level, are presented in parentheses.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

Table B.11: Intent to treat estimates with controls for neighborhood characteristics

	(1)	(2)	(3)
<b>Panel A: All Arrests</b>			
Post voucher service	0.000460 (0.000964)	0.000506 (0.000962)	0.000608 (0.000965)
<b>Panel B: Violent Arrests</b>			
Post voucher service	0.000676* (0.000347)	0.000680* (0.000348)	0.000693** (0.000352)
<b>Panel C: Drug Arrests</b>			
Post voucher service	0.000133 (0.000379)	0.000166 (0.000382)	0.000198 (0.000383)
<b>Panel D: Financial Arrests</b>			
Post voucher service	0.000157 (0.000424)	0.000189 (0.000423)	0.000230 (0.000430)
Observations	85690	85690	85690
Individuals	4510	4510	4510
Quarter FE	Yes	Yes	Yes
Main controls	Yes	Yes	Yes
Census Tract controls	No	Yes	Yes
Dummy for missing Census Tract controls	No	Yes	Yes
Crime controls	No	No	Yes
Dummy for missing crime controls	No	No	Yes

Notes: Each column within a panel represents a separate regression estimating equation 2.2 with a different set of control variables. Main controls include age at the time of the lottery, number of bedrooms and a dummy indicating arrest in the crime category in the 5 years prior to the lottery. Demographic controls include percent black, percent Hispanic, unemployment rate, median household income and poverty rate for the census tract of the individual's application address. Crime controls include rates for overall crime, violent and property crimes per 1000 people in the police division of the individual's application address. To maintain the number of observations constant across specifications, we include dummy variables indicating whether the demographic or crime controls are missing. The dependent variables in Panels A through D are dummy variables indicating an arrest in the person-quarter for any offense, violent offense, drug related offense, and financially motivated offense respectively. Robust standard errors, clustered at the individual level, are presented in parentheses.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

Table B.12: Effects of voucher service on crime - For individuals registered to vote in 2012

	(1)	(2)
<b>Panel A: All Arrests</b>		
Post voucher service	0.00103 (0.00122)	0.00106 (0.00121)
<b>Panel B: Violent Arrests</b>		
Post voucher service	0.000660 (0.000435)	0.000647 (0.000432)
<b>Panel C: Drug Arrests</b>		
Post voucher service	0.000319 (0.000490)	0.000392 (0.000487)
<b>Panel D: Financial Arrests</b>		
Post voucher service	0.000623 (0.000537)	0.000618 (0.000535)
Observations	59907	59907
Individuals	3153	3153
Quarter FE	Yes	Yes
Controls	No	Yes

Notes: Each column within a panel represents a separate regression estimating equation 2 for the subset of voucher recipients that were registered to vote in 2012 in Houston. The dependent variables in Panels A through D are dummy variables indicating an arrest in the person-quarter for any offense, violent offense, drug related offense, and financially motivated offense respectively. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the crime category in the 5 years prior to the lottery. Robust standard errors, clustered at the individual level, are presented in parentheses.



Table B.13: Effect of voucher service on crime - Additional subgroups

	Age		Change of address	
	Age > 30 (1)	Age ≤ 30 (2)	Non-movers (3)	Movers (4)
<b>Panel A: All arrests</b>				
Post Voucher Service	-0.000989 (0.00121)	0.00185 (0.00152)	0.00280** (0.00124)	-0.00242** (0.00109)
Pre-lottery mean	0.0049	0.0061	0.0033	0.0042
<b>Panel B: Violent arrests</b>				
Post Voucher Service	0.000721* (0.000414)	0.000562 (0.000561)	0.000611 (0.000476)	-0.000142 (0.000330)
Pre-lottery mean	0.0005	0.0009	0	0.0008
<b>Panel C: Drug arrests</b>				
Post Voucher Service	-0.000190 (0.000657)	0.000509 (0.000358)	0.000696 (0.000523)	-0.000640* (0.000375)
Pre-lottery mean	0.0018	0.0006	0	0.0007
<b>Panel D: Financial arrests</b>				
Post Voucher Service	0.000394 (0.000424)	-0.000118 (0.000749)	0.000565 (0.000538)	-0.000233 (0.000529)
Pre-lottery mean	0.0004	0.001	0	0.0008
Observations	43947	41743	7106	41876
Individuals	2313	2197	374	2204
Quarter FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Each column within a panel represents a separate regression estimating equation 2.2 within a subgroup. Every pair of columns present heterogeneous effects of the vouchers within subgroups by criminal history, gender, and change of address respectively. The dependent variables in Panels A through D are dummy variables indicating an arrest in the person-quarter for any offense, violent offense, drug related offense, and financially motivated offense respectively. Pre-lottery mean is the mean of quarterly probability of arrest in the crime category for the particular subgroup from the year 2006. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the crime category in the 5 years prior to the lottery. Robust standard errors, clustered at the individual level, are presented in parentheses. Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

## APPENDIX C

Figure C.1: Distribution of the victim-offender relationship and circumstance of homicide  
– In cases with female victim aged 18 to 60, male offender, and not during a robbery

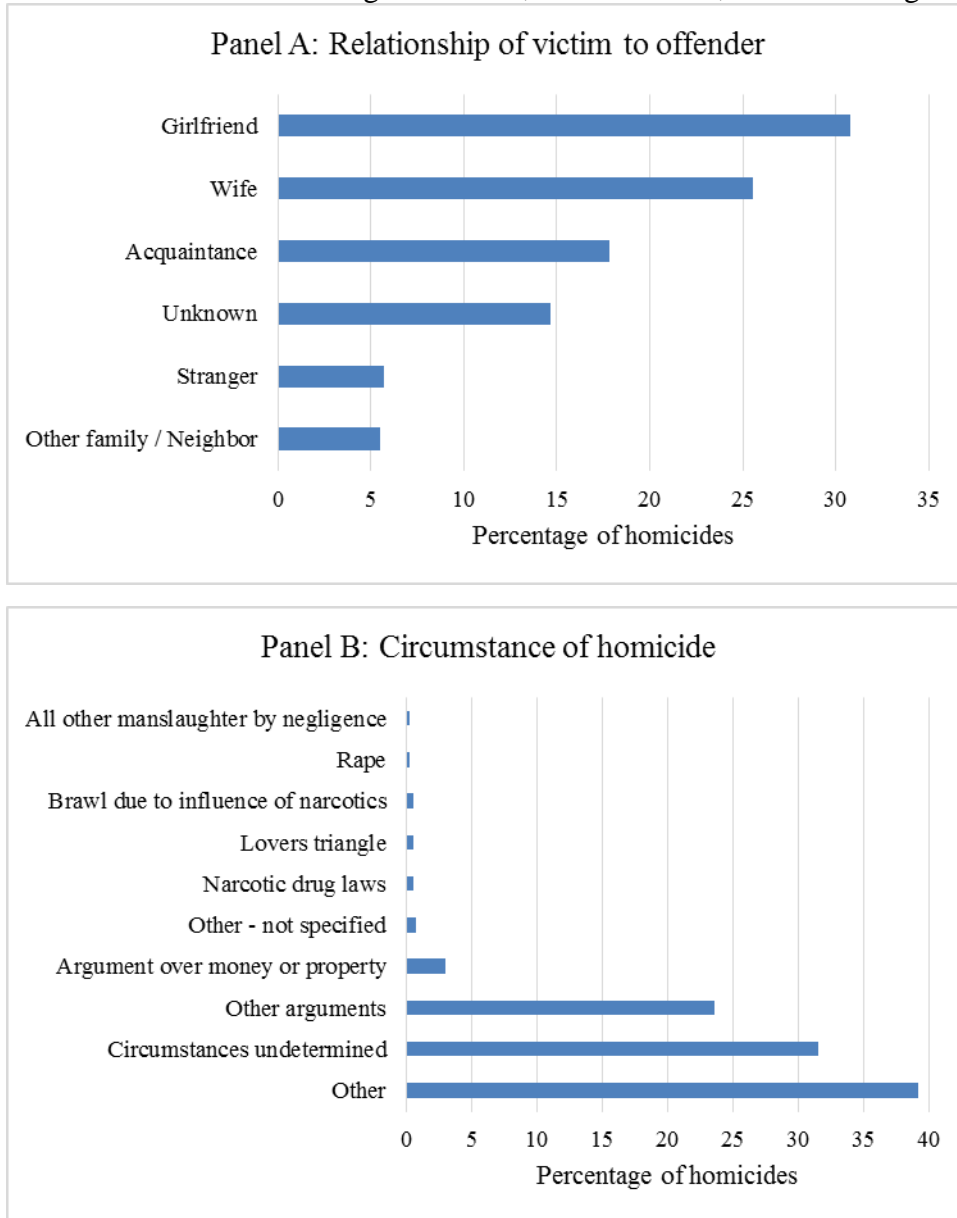
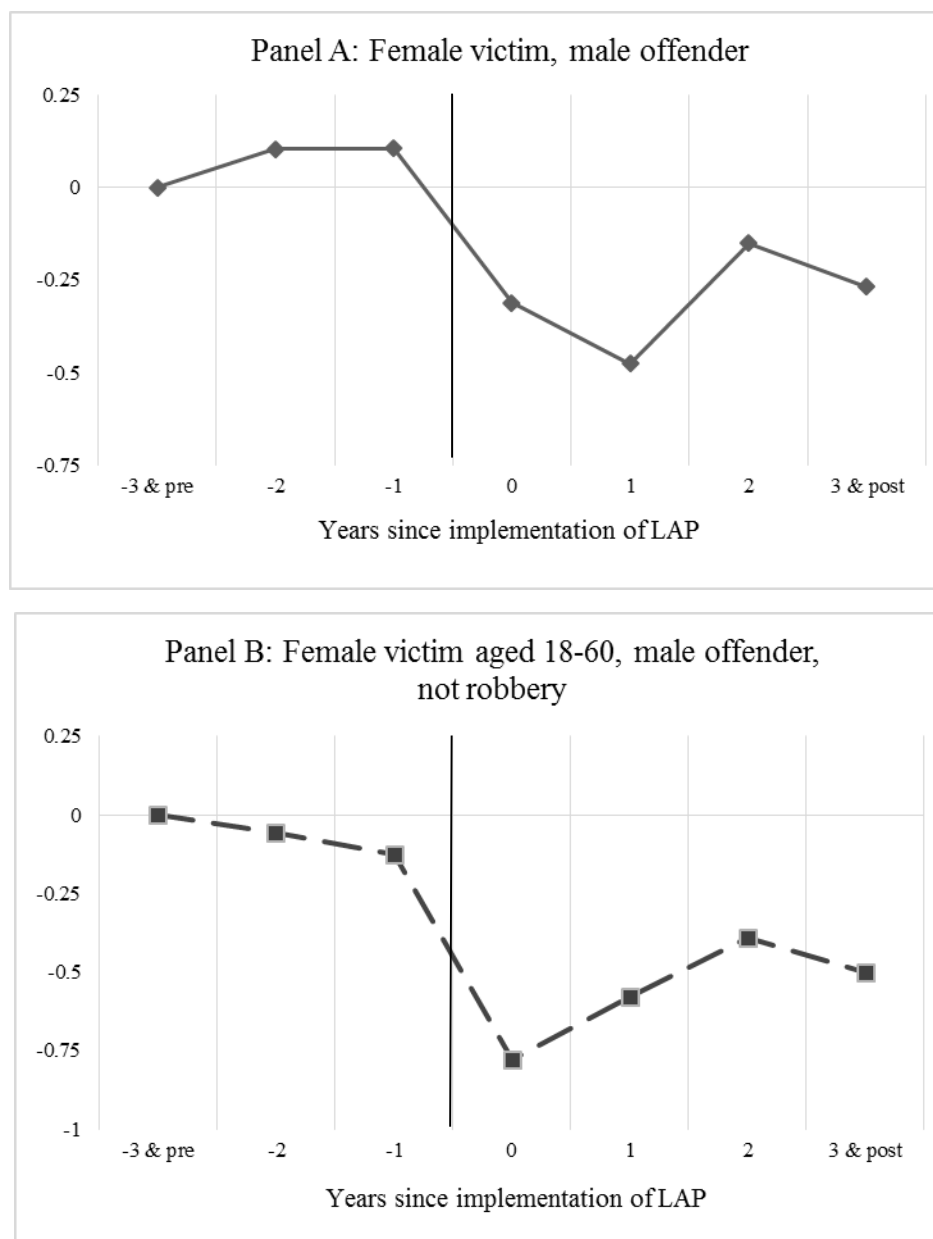



Figure C.2: Divergence in log homicide rates with female victims and male offenders before and after adoption of LAP




Notes: The figure shows the coefficients for the leading indicators and lagged treatment effects from negative binomial regressions, accounting for agency and year fixed effects and covariates. Time-varying controls include median income, poverty rate, unemployment rate, demographics, policing rate and control for the institution of a fatality review team in the county. All coefficients are estimated relative to the difference between the treatment and control groups in the period 3 or more years before treatment. Hence the first data point is fixed to zero.

Figure C.3: Lethality Screen document



## **DOMESTIC VIOLENCE LETHALITY SCREEN FOR FIRST RESPONDERS**



Officer:	Date:	Case #:
Victim:	Offender:	
<input type="checkbox"/> Check here if victim did not answer any of the questions.		
▶ A "Yes" response to any of Questions #1-3 automatically triggers the protocol referral.		
1. Has he/she ever used a weapon against you or threatened you with a weapon?	<input type="checkbox"/> Yes	<input type="checkbox"/> No <input type="checkbox"/> Not Ans.
2. Has he/she threatened to kill you or your children?	<input type="checkbox"/> Yes	<input type="checkbox"/> No <input type="checkbox"/> Not Ans.
3. Do you think he/she might try to kill you?	<input type="checkbox"/> Yes	<input type="checkbox"/> No <input type="checkbox"/> Not Ans.
▶ Negative responses to Questions #1-3, but positive responses to at least four of Questions #4-11, trigger the protocol referral.		
4. Does he/she have a gun or can he/she get one easily?	<input type="checkbox"/> Yes	<input type="checkbox"/> No <input type="checkbox"/> Not Ans.
5. Has he/she ever tried to choke you?	<input type="checkbox"/> Yes	<input type="checkbox"/> No <input type="checkbox"/> Not Ans.
6. Is he/she violently or constantly jealous or does he/she control most of your daily activities?	<input type="checkbox"/> Yes	<input type="checkbox"/> No <input type="checkbox"/> Not Ans.
7. Have you left him/her or separated after living together or being married?	<input type="checkbox"/> Yes	<input type="checkbox"/> No <input type="checkbox"/> Not Ans.
8. Is he/she unemployed?	<input type="checkbox"/> Yes	<input type="checkbox"/> No <input type="checkbox"/> Not Ans.
9. Has he/she ever tried to kill himself/herself?	<input type="checkbox"/> Yes	<input type="checkbox"/> No <input type="checkbox"/> Not Ans.
10. Do you have a child that he/she knows is not his/hers?	<input type="checkbox"/> Yes	<input type="checkbox"/> No <input type="checkbox"/> Not Ans.
11. Does he/she follow or spy on you or leave threatening messages?	<input type="checkbox"/> Yes	<input type="checkbox"/> No <input type="checkbox"/> Not Ans.
▶ An officer may trigger the protocol referral, if not already triggered above, as a result of the victim's response to the below question, or whenever the officer believes the victim is in a potentially lethal situation.		
Is there anything else that worries you about your safety? (If "yes") What worries you?		
Check one: <input type="checkbox"/> Victim screened in according to the protocol <input type="checkbox"/> Victim screened in based on the belief of officer <input type="checkbox"/> Victim did not screen in		
If victim screened in: After advising her/him of a high danger assessment, <input type="checkbox"/> Yes <input type="checkbox"/> No did the victim speak with the hotline counselor?		

*Note: The questions above and the criteria for determining the level of risk a person faces is based on the best available research on factors associated with lethal violence by a current or former intimate partner. However, each situation may present unique factors that influence risk for lethal violence that are not captured by this screen. Although most victims who screen "positive" or "high danger" would not be expected to be killed, these victims face much higher risk than that of other victims of intimate partner violence.*

Table C.1: List of agencies in Maryland with at least 10,000 population and their LAP implementation dates

Agency	County	LAP start date	In sample	Treated
Kent County Sheriff's Office	Kent	October 2005	Yes	Yes
Cambridge Police Department	Dorchester	November 2005	Yes	Yes
Frederick Police Department	Frederick	November 2005	Yes	Yes
Hagerstown Police Department	Washington	November 2005	Yes	Yes
Harford County Sheriff's Office	Harford	November 2005	Yes	Yes
Queen Anne's County Sheriff's Office	Queen Anne's	November 2005	Yes	Yes
Easton Police Department	Talbot	February 2006	Yes	Yes
Garrett County Sheriff's Office	Garrett	February 2006	Yes	Yes
Talbot County Sheriff's Office	Talbot	February 2006	Yes	Yes
Baltimore Police Department	Baltimore (city)	April 2006	Yes	Yes
Calvert County Sheriff's Office	Calvert	May 2006	Yes	Yes
Cumberland Police Department	Allegany	May 2006	Yes	Yes
Washington County Sheriff's Office	Washington	May 2006	Yes	Yes
Cecil County Sheriff's Office	Cecil	July 2006	Yes	Yes
Elkton Police Department	Cecil	July 2006	Yes	Yes
Frederick County Sheriff's Office	Frederick	July 2006	Yes	Yes
Annapolis Police Department	Anne Arundel	July 2007	Yes	Yes
Anne Arundel County Sheriff's Office	Anne Arundel	July 2007	Yes	Yes
Caroline County Sheriff's Office	Caroline	2007	Yes	Yes
Dorchester County Sheriff's Office	Dorchester	2007	Yes	Yes
Howard County Sheriff's Office	Howard	January 2008	Yes	Yes
Gaithersburg Police Department	Montgomery	August 2008	Yes	Yes
Montgomery County Sheriff's Office	Montgomery	August 2008	Yes	Yes
Rockville Police Department	Montgomery	August 2008	Yes	Yes
Takoma Park Police Department	Montgomery	August 2008	Yes	Yes
St. Mary's County Sheriff's Office	St. Mary's	2008	Yes	Yes
Baltimore County Sheriff's Office	Baltimore	May 2009	Yes	Yes
Somerset County Sheriff's Office	Somerset	May 2009	Yes	Yes
State Police: Carroll County	Carroll	July 2009	Yes	Yes
Westminster Police Department	Carroll	July 2009	Yes	Yes
Charles County Sheriff's Office	Charles	October 2009	Yes	Yes
Aberdeen Police Department	Harford	December 2009	Yes	Yes
Wicomico County Sheriff's Office	Wicomico	2009	Yes	Yes
Worcester County Sheriff's Office	Worcester	2009	Yes	Yes
College Park Police Department	Prince George's	December 2012	Yes	No
Greenbelt Police Department	Prince George's	December 2012	Yes	No
Hyattsville Police Department	Prince George's	December 2012	Yes	No

Laurel Police Department	Prince George's	December 2012	Yes	No
Prince George's County Sheriff's Office	Prince George's	December 2012	Yes	No
Havre De Grace Police Department	Harford	Unknown	No	-
Ocean Pines Police Department	Worcester	Unknown	No	-
Salisbury Police Department	Wicomico	Unknown	No	-
State Police: Allegany County	Allegany	Unknown	No	-

Notes: The start dates were compiled by the author from different sources such as newsletters from MNADV, police department websites, and emails from police departments and associated domestic violence helplines. The last two columns provide information about the treatment status of each agency in the sample and within the period of study. For four of the agencies with a population of 10,000 or more, the LAP implementation date is not known. So, these agencies are excluded from all the analyses. For 5 more agencies, the implementation year is known but the month is not. In these cases, July (mid-year) is assumed to be the month of implementation.

Table C.2: Lethality screening and counselling statistics from 2006 to 2009

	2006	2007	2008	2009	Four-year average
<b><u>Panel A: Overall Statistics</u></b>					
Number of agencies	21	43	68	88	
Population being served (in 1000)	807	1725	3198	4228.5	
Number of lethality screens	1839	3304	6788	10497	
Number of high danger victims	990	1923	3713	5443	
Number of non-high danger victims	698	1179	2589	4315	
Number that refused the screening	151	202	486	739	
Number that spoke to the counselor	531	1030	2207	3322	
Number that sought services	158	263	621	1030	
<b><u>Panel B: Statistics for every 100,000 population</u></b>					
Number of lethality screens	228	192	212	248	220
Number of high danger victims	123	111	116	129	120
Number that spoke to the counselor	66	60	69	79	69
Number that sought services	20	15	19	24	20

Notes: All statistics are from the Lethality Assessment information packet prepared by MNADV in 2010

Table C.3: Summary statistics

	Mean	Std. Dev.	Range
<b><u>Panel A: Agency-level dependent variables: Homicides per 100k population</u></b>			
All homicides	4.17	7.36	0 - 46.78
Male victim	3.25	6.65	0 - 43.09
Female victim	0.93	1.77	0 - 9.19
Female victim and male offender	0.67	1.50	0 - 9.19
Female victim aged 18 to 60, male offender, not a robbery	0.44	1.18	0 - 9.19
Female victim was wife/girlfriend of offender	0.29	0.98	0 - 9.19
Female victim was girlfriend of offender	0.14	0.68	0 - 9.19
Female victim was wife of offender	0.16	0.72	0 - 8.13
Female victim aged 18 to 60, was an acquaintance of male offender, not a robbery	0.10	0.52	0 - 5.8
<b><u>Panel B: County-level control variables</u></b>			
Population (within agency jurisdiction)	138431	228072	10877 - 962146
% Black	22.5	20.2	0.4 - 66.2
% White	72.0	21.4	26.1 - 98.9
% Male	48.8	1.3	46.6 - 53.8
% White male	35.4	10.3	13.7 - 49
% Black male	10.8	9.3	0.3 - 30.6
Poverty rate	8.9	4.2	3.7 - 26.2
Unemployment rate	5.1	2.0	2.5 - 13.6
Median household income (\$1000)	60.6	17.2	28.2 - 102
Number of police per 100k population	165.3	71.0	26.7 - 508.1
Fatality review team in the county	0.33	0.47	0 - 1

Notes: Summary statistics have been calculated from 468 observations which include data from 39 agencies over 12 years. All the dependent variables are annual counts of homicide per 100,000 population calculated from the FBI's Supplementary Homicide Reports. Except for population, the rest of the independent variables are at the county level. Agency level population data are from and population estimates of the FBI. Demographic information is derived from county-level population estimates from the Census. Poverty and income information are from the Small Area Income and Poverty Estimates. Unemployment rates are from the Local Area Unemployment Statistics of the Bureau of Labor Statistics. Police counts are from FBI's UCR.

Table C.4: Effect of LAP on female homicide victimization

	1	2	3	4	5	6	7
<b>Panel A: Log homicide rate - Female victim</b>							
LAP	-0.117 (0.130)	-0.110 (0.0816)	-0.118 (0.0886)	-0.200** (0.102)	-0.190* (0.110)	-0.208** (0.0961)	-0.328* (0.194)
LAP t-1						-0.0292 (0.139)	
<b>Panel B: Log homicide rate - Female victim and male offender</b>							
LAP	-0.366*** (0.136)	-0.318*** (0.118)	-0.355*** (0.129)	-0.450*** (0.140)	-0.444*** (0.147)	-0.432*** (0.137)	-0.477* (0.263)
LAP t-1						0.0188 (0.197)	
<b>Panel C: Log homicide rate - Female victim aged 18 to 60, male offender, not a robbery</b>							
LAP	-0.381*** (0.100)	-0.422*** (0.162)	-0.421** (0.174)	-0.433** (0.214)	-0.416** (0.212)	-0.489** (0.219)	-0.575* (0.336)
LAP t-1						-0.108 (0.260)	
Observations	468	468	468	468	468	468	468
Agencies	39	39	39	39	39	39	39
Agency and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls		Yes	Yes	Yes	Yes	Yes	Yes
Economic controls			Yes	Yes	Yes	Yes	Yes
Policing controls				Yes	Yes	Yes	Yes
Policy controls					Yes	Yes	Yes
Agency specific linear time trends							Yes

Notes: Each column in each panel represents a separate regression. The unit of observation is an agency-year. Robust standard errors are clustered at the agency level. Time-varying controls include median income, poverty rate, unemployment rate, demographics, policing rate and a dummy variable indicating the presence of a domestic violence fatality review team in the county.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level



Table C.5: Effect of LAP on female homicide victimization – By relationship with male offender

	1	2	3	4	5	6	7
<b>Panel A: Log homicide rate - Female victim was girlfriend of male offender</b>							
LAP	-0.602*	-0.569**	-0.683**	-1.086***	-1.040***	-1.119***	-0.802
	(0.323)	(0.281)	(0.266)	(0.329)	(0.317)	(0.350)	(0.500)
LAP t-1						-0.0980	
						(0.303)	
<b>Panel B: Log homicide rate - Female victim was wife of male offender</b>							
LAP	0.382	0.329	0.311	0.546	0.669*	0.635	0.923
	(0.335)	(0.344)	(0.367)	(0.382)	(0.390)	(0.427)	(0.639)
LAP t-1						-0.0595	
						(0.228)	
<b>Panel C: Log homicide rate - Female victim aged 18 to 60, was acquaintance of male offender, not a robbery</b>							
LAP	-0.874**	-0.866*	-0.884**	-0.895*	-1.015**	-1.171**	-1.052
	(0.423)	(0.451)	(0.438)	(0.466)	(0.448)	(0.547)	(1.060)
LAP t-1						-0.249	
						(0.511)	
Observations	468	468	468	468	468	468	468
Agencies	39	39	39	39	39	39	39
Agency and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls		Yes	Yes	Yes	Yes	Yes	Yes
Economic controls			Yes	Yes	Yes	Yes	Yes
Policing controls				Yes	Yes	Yes	Yes
Policy controls					Yes	Yes	Yes
Agency specific linear time trends							Yes

Notes: Each column in each panel represents a separate regression. The unit of observation is an agency-year. Robust standard errors are clustered at the agency level. Time-varying controls include median income, poverty rate, unemployment rate, demographics, policing rate and control for the institution of a fatality review team in the county. Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

Table C.6: Falsification - Effect of LAP on all homicides and male homicide victimization

	1	2	3	4	5	6	7
<b>Panel A: Log homicide rate</b>							
LAP	0.0614** (0.0271)	0.0389 (0.0664)	0.0452 (0.0657)	-0.0210 (0.0775)	-0.0196 (0.0768)	-0.0501 (0.0746)	0.0685 (0.122)
LAP t-1						-0.0484 (0.0563)	
<b>Panel B: Log homicide rate - Male victim</b>							
LAP	0.0870* (0.0488)	0.0352 (0.0685)	0.0470 (0.0688)	0.0105 (0.0889)	0.0107 (0.0877)	-0.0360 (0.0871)	0.191 (0.121)
LAP t-1						-0.0706 (0.0523)	
Observations	468	468	468	468	468	468	468
Agencies	39	39	39	39	39	39	39
Agency and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls		Yes	Yes	Yes	Yes	Yes	Yes
Economic controls			Yes	Yes	Yes	Yes	Yes
Policing controls				Yes	Yes	Yes	Yes
Policy controls					Yes	Yes	Yes
Agency specific linear time trends							Yes

Notes: Each column in each panel represents a separate regression. The unit of observation is an agency-year. Robust standard errors are clustered at the agency level. Time-varying controls include median income, poverty rate, unemployment rate, demographics, policing rate and control for the institution of a fatality review team in the county.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

Table C.7: County-level analysis

	1	2	3	4
<b><u>Panel A: Log homicide rate - Female victim</u></b>				
LAP	-0.0961 (0.108)	-0.144 (0.116)	-0.171* (0.0973)	-0.210* (0.124)
<b><u>Panel B: Log homicide rate - Female victim and male offender</u></b>				
LAP	-0.389*** (0.141)	-0.436*** (0.134)	-0.481*** (0.152)	-0.509*** (0.143)
<b><u>Panel C: Log homicide rate - Female victim aged 18 to 60, male offender, not a robbery</u></b>				
LAP	-0.392*** (0.103)	-0.381** (0.192)	-0.508*** (0.106)	-0.493*** (0.191)
Observations	288	288	288	288
Counties	24	24	24	24
County and year fixed effects	Yes	Yes	Yes	Yes
Controls		Yes		Yes
County treated when largest agency implements LAP	Yes	Yes		
County treated proportional to the fraction of the population treated			Yes	Yes

Notes: Each column in each panel represents a separate regression. In columns 1 and 2, the treatment is defined as the proportion of the year during which the largest law enforcement agency in the county is treated. In columns 3 and 4, the treatment is scaled by the proportion of the county population that is treated. The unit of observation is an agency-year. Robust standard errors are clustered at the county level. Time-varying controls include median income, poverty rate, unemployment rate, demographics, policing rate and control for the institution of a fatality review team in the county.

Significance: \* 10% level; \*\* 5% level; \*\*\* 1% level

Table C.8: LAP implementation dates for agencies by county

County	Agency name	Largest known agency	Percentage of population in agency	LAP start date
Allegany	Cumberland Police Department	Yes	29	May 2006
Anne Arundel	Anne Arundel County Police Department	Yes	92	July 2007
Anne Arundel	Annapolis Police Department	No	7	July 2007
Baltimore	Baltimore County Police Department	Yes	100	May 2009
Baltimore (city)	Baltimore Police Department	Yes	100	April 2006
Calvert	Calvert County Sheriff's Office	Yes	99	May 2006
Caroline	Caroline County Sheriff's Office	Yes	69	2007
Carroll	State Police: Carroll County Police	Yes	77	July 2009
Carroll	Westminster Police Department	No	11	July 2009
Cecil	Cecil County Sheriff's Office	Yes	77	July 2006
Cecil	Elkton Police Department	No	14	July 2006
Charles	Charles County Sheriff's Office	Yes	94	October 2009
Dorchester	Cambridge Police Department	No	36	November 2005
Dorchester	Dorchester County Sheriff's Office	Yes	58	2007
Frederick	Frederick Police Department	No	27	November 2005
Frederick	Frederick County Sheriff's Office	Yes	67	July 2006
Garrett	Garrett County Sheriff's Office	Yes	94	February 2006
Harford	Harford County Sheriff's Office	Yes	83	November 2005
Harford	Aberdeen Police Department	No	6	December 2009
Howard	Howard County Police Department	Yes	99	January 2008
Kent	Kent County Sheriff's Office	Yes	68	October 2005
Montgomery	Montgomery County Police Department	Yes	98	August 2008
Montgomery	Gaithersburg Police Department	No	6	August 2008
Montgomery	Rockville Police Department	No	5	August 2008
Montgomery	Takoma Park Police Department	No	2	August 2008
Prince George's	Prince George's County Police Department	Yes	85	December 2012
Prince George's	College Park Police	No	3	December 2012
Prince George's	Greenbelt Police Department	No	3	December 2012
Prince George's	Hyattsville Police Department	No	2	December 2012
Prince George's	Laurel Police Department	No	2	December 2012
Queen Anne's	Queen Anne's County Sheriff's Office	Yes	95	November 2005
Somerset	Somerset County Sheriff's Office	Yes	23	May 2009
St. Mary's	St. Mary's County Sheriff's Office	Yes	100	2008
Talbot	Talbot County Sheriff's Office	Yes	59	February 2006
Talbot	Easton Police Department	No	35	February 2006
Washington	Hagerstown Police Department	No	28	November 2005
Washington	Washington County Sheriff's Office	Yes	69	May 2006
Wicomico	Wicomico County Sheriff's Office	Yes	65	2009
Worcester	Worcester County Sheriff's Office	Yes	40	2009